Field Experiments on Cooperation Behavior

Inaugural-Dissertation

zur

Erlangung der wirtschaftswissenschaftlichen Doktorwürde

des Fachbereichs Wirtschaftswissenschaften

der Philipps-Universität Marburg

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Einreichungstermin:	16.12.2020
Prüfungstermin:	08.03.2021
Erscheinungsort:	Marburg
Hochschulkennziffer:	1180

Acknowledgements

First of all, I would hereby like to thank all those who have supported me during the past years of preparing and writing this doctoral thesis. That is, first and foremost, of course, Prof. Dr. Michael Kirk who, back in 2015, selected me for the position as a research fellow and PhD student at his chair for development- and cooperative economics. He not only carefully supervised me during the process of finding suitable research topics, collecting the data and writing the results, but he also gave me the freedom to select research questions for my thesis that I find interesting and enjoyed working on. He further made the whole project possible by installing me within the SASSCAL research program, so I could carry out the field work in Namibia that builds the basis for my thesis. Funding of the project and my research was therefore provided by the Southern African Science Service Center for Climate Change and Adaptive Land-Use Management (SASSCAL) through the German Federal Ministry for Education and Research [grant number 01LG1201B] for which I am also thankful. Further I would like to thank my second supervisor, Prof. Dr. Björn Vollan, our secretary, Bärbel Dönges, as well as all of my colleagues for their advice and support. These are, in alphabetical order, Boban Aleksandrovic, Lawrence Brown, Max Burger, Dr. Thomas Dufhues, Patrick Hoffmann, Dr. Lukas Kampenhuber, Dr. Olayinka Kareem, Matthias Mayer, Sascha Mierzwa, Marco Nilgen, Dr. Oliver Ried, Edgard Robleto, Christoph Rößler, Dr. Ivo Steimanis, Dr. Tobias Vorlaufer, Martin Voß and Johannes Zahner. Special thanks go to Adrian Pourviseh and Dr. Tobias Vorlaufer, who went to Namibia with me and were of invaluable support. On site, no field research would have been possible without the help of our local assistants Anastasia, Blondy, Cypriaan, James and Moses. I would further like to thank all Namibian officials, stakeholders, local authorities and villagers who participated in our research and thereby made the whole project feasible. I am also grateful to all participants of conferences and workshops in Marburg, Tehran, Hamburg, Washington D.C., Berlin and Kiel who gave me useful advice and comments on my research. Finally, I have to thank my friends and family for their direct and indirect support and patience. And, lastly, my students, who I not only enjoyed teaching but who also helped me, through questions, curiosity and their original ideas, to advance also my own knowledge and made me improve my presentation and teaching skills. I hope I have been a good teacher to them and was able to transfer, on some occasions, a bit more than just textbook economic knowledge.

Summary in German

(Inhaltliche Zusammenfassung in deutscher Sprache)

Die vorliegende Doktorarbeit ist als kumulative Dissertation aufgebaut und umfasst, nach einer kurzen Einleitung, vier wissenschaftliche Aufsätze. Thematisch befassen sich alle dieser Aufsätze damit, unter welchen Umständen Menschen miteinander kooperieren. Drei davon basieren auf ökonomischen Feldexperimenten, die im Rahmen des SASSCAL-Forschungsprojektes in der Kavango-Region in Namibia durchgeführt wurden. Der Hintergrund der jeweiligen Fragestellungen ist, neben dem rein akademischen Erkenntnisgewinn, primär die Problematik der nachhaltigen Nutzung natürlicher Ressourcen. Im konkreten Fall handelt es sich dabei um Waldflächen, welche zwecks Landgewinn abgeholzt werden, was langfristig lokale sowie globale negative Externalitäten nach sich zieht. Die Fragestellungen und Ergebnisse der einzelnen Aufsätze sind wie folgt:

Der erste Aufsatz ergründet, wie sich Ressourcenknappheit auf das Entnahmeverhalten der Nutzer auswirkt. Dazu werden experimentell zwei Zustände eingeführt, einer in dem die verfügbaren Ressourcen ausreichend vorhanden sind, sodass sich kein Nutzer Sorgen machen muss, vor Ende des Experimentes keine Ressourcen mehr entnehmen zu können. Im anderen Zustand steht nur die Hälfte der Ressourcen zur Verfügung und dies hat zur Folge, dass der Bestand vorzeitig aufgebraucht werden kann. Beide Situationen sind als soziales Dilemma modelliert, konkret als Allmendegut und in jedem Fall ist es sozial optimal, die Ressourcen zu schonen. Individuell jedoch kann jeder Teilnehmer den eigenen Gewinn erhöhen, indem so viel wie nur möglich entnommen wird, unabhängig davon, wie die anderen Teilnehmer sich verhalten. Eine theoretische Vorhersage, welche Auswirkungen die beiden Zustände, Knappheit und Abundanz, auf das Entnahmeverhalten haben, ist in diesem Fall schwierig. Es ist sowohl vorstellbar, dass Knappheit zu einer erhöhten Extraktionsrate der Ressourcen führt, da Teilnehmer sich frühzeitig Ressourcen sichern wollen bevor diese aufgebraucht sind. Es ist jedoch auch denkbar, dass Knappheit zu einem vorsichtigeren Umgang mit den verbleibenden Ressourcen führt, da Nutzer sich nun mehr um den Fortbestand derselben sowie den daraus gezogenen sozialen Nutzen sorgen. Erkenntnisse darüber, wie Individuen in derartigen Situationen entscheiden, werden daher anhand der empirischen Beobachtungen im Feldexperiment gewonnen. Tatsächlich stellt sich heraus, dass Knappheit zu einer geringeren Entnahmegeschwindigkeit führt. Es zeigte sich außerdem, dass das Verhalten im Experiment auch davon abhängt, ob die Teilnehmer in einer Region leben, in der tatsächlich Landknappheit herrscht.

Der zweite Aufsatz untersucht den Effekt externer Anreizmechanismen für eine nachhaltigere Ressourcennutzung. Erneut wird hierzu ein Feldexperiment durchgeführt, welches ein Allmendegut modelliert. Teilnehmer entscheiden sich über mehrere Runden hinweg, wie oft sie ein Stück im „experimentellen“ Wald roden möchten, um dort ein neues Feld anzulegen, oder ob sie auf ihren bestehenden Feldern bleiben und geringere Erträge in Kauf nehmen. Es handelt sich also auch hier um ein soziales Dilemma, in dem es für die Allgemeinheit am besten ist, wenn möglichst wenig Wald gerodet wird. Dies entspricht auch der Realität im betroffenen Gebiet und ist den dortigen Menschen auch durchaus bewusst. Aufgrund von vorherrschender Armut nehmen jedoch kurzfristig erhöhte Erträge einen höheren Stellenwert ein als die langfristigen Vorteile, die ein intakter Waldbestand bietet. Aus diesem Grunde bilden sogenannte Zahlungen für Ökosystemdienstleistungen ein besonders interessantes Politikinstrument. Die Idee ist es, das gewünschte Verhalten, im vorliegenden Fall also einen nachhaltigen Umgang mit dem Waldbestand, zu belohnen, beispielsweise durch monetäre Anreize. Dabei können, im Gegensatz zu Maßnahmen zur Bestrafung von unerwünschtem Verhalten, zwei Ziele zugleich erreicht werden, nämlich sowohl die Steuerung zugunsten nachhaltigen Verhaltens als auch eine Komponente zur Bekämpfung von Armut (Bulte et al. 2008; Milder et al. 2010). Der vorliegende Aufsatz vergleicht hierzu sowohl negative (Strafen) als auch positive Zahlungen, wobei letztere in zwei verschiedenen Varianten durchgeführt werden: Zum einen als individuelle Belohnung, was als direktes Gegenstück zu individuellen Strafzahlungen verstanden werden kann. Zum anderen als kollektive Belohnung, welche dementsprechend auch an einen kollektiven Erfolg der Erhaltung des Waldes geknüpft ist. Kollektive Zahlungen für Ökosystemdienstleistungen sind besonders interessant, da sich ein kollektiver Erfolg sehr viel leichter beobachten lässt als das Verhalten jedes einzelnen Akteurs zu überwachen, was immense Transaktionskosten mit sich bringen würde (Engel 2016). Darüber hinaus, wären individuelle Zahlungen in manchen Umfeldern gar nicht möglich, da es keine individuellen Eigentums- oder Nutzungsrechte gibt, anhand derer entsprechende Leistungen und Zahlungen zugeordnet und umgesetzt werden könnten. Dies ist beispielsweise in der Forschungsregion, dem Kavango, größtenteils der Fall. Waldflächen gehören, genau wie Ackerland, formal nicht den jeweiligen Farmern, sondern sind Gemeindeland. Von kollektiven Strafzahlungen als weitere mögliche Maßnahme sehen wir aus ethischen Gründen ab und vermuten, dass dies aus demselben Grund auch wenig Realitätsbezug hätte. Im vorliegenden Aufsatz wird also zuerst die Effektivität der unterschiedlichen Arten von Anreizmechanismen vergleichen, wobei sich herausstellt, dass alle drei getesteten Maßnahmen ähnlich gut wirken. Danach werden die externen Anreize wieder weggenommen, um zu untersuchen, ob kurzzeitige

Programme sich auf die intrinsische Motivation zur nachhaltigen Nutzung auswirken. Hier ergeben sich heterogene Effekte, wobei insbesondere der Wegfall positiver Anreize sich eher negativ auswirken kann.

Der dritte Aufsatz beschäftigt sich mit den bereits angesprochenen kollektiven, positiven Anreizmechanismen. Es stellt nur einen sehr kurzen, theoretischen Beitrag dar und wurde in „Conservation Letters“ als Korrespondenz veröffentlicht. Die Kernaussage ist, dass wenn die Konditionalität der Zahlungen an den kollektiven Erfolg geknüpft ist, automatisch ein (weiteres) soziales Dilemma auf dieser Ebene entsteht. Dies muss jedoch nicht ausschließlich negative Auswirkungen haben.

Der vierte Aufsatz behandelt den Effekt von Kommunikation auf Kooperation. Frühere Studien haben gezeigt, dass eine vorangehende Diskussion zwischen den betroffenen Teilnehmern über die Dilemma-Situation dazu führt, dass deutlich mehr kooperiert wird (Dawes 1980; Sally 1995; Balliet 2010; Ostrom 2010). Obwohl zahlreiche Studien dies bestätigen, wurde bisher nur wenig darüber herausgefunden, warum genau dies so ist. Im Aufsatz wird zunächst ein theoretisches Framework aufgestellt, welches die grundsätzlich möglichen Erklärungen zu diesem Effekt liefert. Danach werden anhand eines weiteren Feldexperimentes verschiedene Hypothesen untersucht und es stellt sich heraus, dass der Kommunikationseffekt in sozialen Dilemmata zum einen auf verbesserte persönliche Beziehungen zwischen den Teilnehmern zurückzuführen ist und zum anderen Individuen dazu neigen, sich an getroffene Abmachungen zu halten. Letzteres führt jedoch nicht, wie man erwarten könnte, zu gegenseitigen, höheren Erwartungen der Kooperation. Die Tendenz, Versprechen einzuhalten scheint vielmehr eine persönliche Präferenz darzustellen, die unabhängig von Erwartungen an andere wirkt.

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Abbreviations

before being used, abbreviations will also be introduced, at least once, in-text

DID = Difference-in-Differences

LPM = Linear Probability Model

OLS = Ordinary Least Squares

PES = Payments for Ecosystem Services

SASSCAL = Southern African Science Service Center for Climate Change and Adaptive Land-Use Management

Preface

1. Introduction

How do individuals decide if they have to choose between a selfish option that increases their own benefit and a cooperative option that is better for everyone around them but entails a personal cost? Likely the answer is “it depends”, which is, without doubt, each and every economist’s favorite answer on pretty much any question asked. But on what exactly does it depend if an individual makes a decision to cooperate or to defect in a social dilemma? By definition, a social dilemma poses a situation where affected participants have to decide between a cooperative and a selfish option (Olson 1965, Dawes 1980). The overall, i.e., social, welfare is maximized if everybody cooperates. On individual level, however, there is always an incentive to defect, regardless of whether the other participants cooperate or not. In the most extreme case, a free-rider benefits from the others’ cooperative efforts while personally deciding to go for the selfish option. In other words, there is a conflict between individual and collective interests.

Typical social dilemma situations in economics are the prisoner’s dilemma, public goods and common-pool resources. As real-world applications there are environmental issues, such as avoiding pollution and the sustainable use of natural resources (Hardin 1968, Dawes 1980). These can pose small- as well as large scale dilemmas, with climate change currently being one of the major challenges humankind has to face. In general economics, cooperation does, for example, become relevant in decisions about trade barriers or in situations where externalities occur. On political level, it could mean voting for parties that aim to increase the overall welfare and not just those pursuing one’s own interests. Integrity of politicians could also be considered a cooperative dilemma: Despite knowing that the whole system can only run well if everybody works towards common goals, individuals might go corrupt for personal gains. Further applications span from peacekeeping, such as avoiding arms races, down to groups working together in everyday life on tasks that require cooperation in order to achieve common goals.

Recently, in the year of global virus pandemic, taking personal measures to prevent the spread of air-borne Covid-19 infections poses just another large-scale social dilemma: While wearing a mask oneself causes some inconvenience and only minor protection against contracting the disease, the combined effect of everyone wearing masks can make a great impact on reducing infection rates. However, doing so may stand in contrast with maximizing one’s own benefit. Individuals can free-ride on their peers’ effort of wearing masks and taking other measures,

such as reducing social contacts or (soon) getting vaccinated, without making any endeavor themself.

From a broader perspective, the general willingness to help each other, particularly when in need can be seen as a norm of (intertemporal) cooperativeness, even without any altruistic motivation. In this sense, the solution to social dilemmas can also be boiled down to Kant's categorical imperative: One should always and only act according to principles that one desires everybody else to follow, too (Kant 1785).

In economics, however, the concept of the *homo oeconomicus* predicts entirely rational and selfish behavior as, presumably, all utility is drawn from one's own benefit only (cf. Mill 1836). Unanimous defection would therefore be the dominant behavior in social dilemma situations. But can humans, essentially, be considered selfish and rational beings? If that was the case, then everybody's behavior would be entirely predictable, all decisions could be forecasted and any regulative policy measure designed accordingly. Fortunately, though, it was rather unambiguously found that humans do have social preferences: They care for each other's welfare, show aversion to inequalities, cooperate in order to reach greater achievements, and sometimes even follow purely altruistic motives (Smith 1759; Ledyard 1995; Zelmer 2003; Fehr and Fischbacher 2003; Henrich et al. 2005; Batson 2011).

Cooperation must further not necessarily be an irrational strategy if the focus is on long term gains and development rather than on short term profits. For example, in (indefinitely) repeated cooperative interactions, establishing cooperation will eventually result in higher payoffs for everyone (cf. Pettit and Sugden 1989; Bó 2005; Normann and Wallace 2012).[1] According to the concept of conditional cooperation, defection of single participants might cause fragile cooperative norms to fall apart resulting, overall, in losses, also for those who tried to free-ride (Fischbacher et al. 2001; Fehr et al. 2002; Croson et al. 2005; Croson 2007; Kocher et al. 2008; Chaudhuri 2011). Further, free-riders might face direct punishment by others and loss of reputation, making the selfish behavior unattractive (Boyd and Richerson 1992; Fehr and Gaechter 2000; Fowler 2005; Henrich et al. 2006; Boyd et al. 2010; Balliet et al. 2011).

Cooperation also becomes a rational option if one does not only look at individuals interacting with each other but also at groups interacting with as well as competing against other groups: Within each group, surely, it remains individually optimal to behave selfishly in the short-term.

[1] Game theory predicts, based on backward induction, a collapse of cooperation in finitely repeated cooperative interactions, but this prediction is not necessarily backed by empirical observations (cf. Pettit and Sugden 1989; Andreoni and Miller 1993; Norman and Wallace 2012).

Groups consisting of too many selfish, uncooperative individuals however, will be disadvantaged against cooperative groups in relevant tasks, such as acquisition of resources, production of goods or even direct, violent conflicts. This is an aspect that might have been quite obvious and relevant for the longest part of mankind's history, when humans used to live in smaller groups and communities. From an evolutionary point of view, less successful groups will emblematically and inevitably "die out", unless they change their behavior or find new ways of survival (Darwin 1859). Nowadays, however, some of the aforementioned dilemmas take place on such a large scale that competition between groups becomes meaningless as the whole humankind is affected.

2. Methodology

In order to advance insights on decision making in social dilemmas, I present, in this thesis, a few studies that experimentally analyze cooperation in different specific situations and environments. Results may, consequently, help in predicting individual as well as collective behavior and may be of use for planning of respective policies.

Where theoretical models fail to explain human behavior, making empirical observations in controlled and uncontrolled environments can fill the gap and help learning about patterns and interrelations. The observation of real-world data, however, is usually, with standard econometric methods, limited to discovering correlations. Such correlations can then, through deduction, lead to insights about causal effects, but can often not be proven as the direction could be reversed or the influence of missing variables might have been neglected.

Especially in the discipline of behavioral economics, observing and interpreting real-world behavior must be considered as rather cumbersome or even problematic, as along with the plurality of unobservable side effects and correlations it is difficult to clearly isolate single influential factors. Making statements about causal relations are therefore difficult and hardly ever definite.

Methodically, I therefore base my three main papers on experimental approaches in order to clearly isolate and identify such behavioral effects. The particular advantage is that experiments allow creating a controlled environment.[2] Following the methods of natural sciences, all

[2] Detailed explanation on the reasons for and advantages of conducting economic experiments, and (lab-in-the-) field experiments in particular, can be found in ample, available literature and are therefore only briefly summarized here (e.g., Smith and Williams 1992; Smith 1992; Kagel and Roth 1995; Harrison and List 2004; Carpenter et al. 2005; Cardenas and Carpenter 2008; Croson and Gaechter 2010; Viceisza 2012; Viceisza 2016).

parameters except one are kept constant. Therefore, despite empirically still only observing correlations, effects that follow an induced change in just the one variable of interest (treatment) can be interpreted as causal (as long as the randomization of the sample can be assumed as valid).

The backbone of experimental (and most other) empirical research designs is, essentially, comparing at least two different conditions with each other in order to draw conclusions from observed correlations. If a difference in the dependent variable is observed it can then be attributed to the (induced or observed) variation of independent factors between the respective conditions. There are two generic types of comparisons that common research designs allow: Firstly, the within-subject perspective, where the change over time within the same entity (e.g., country or individual) is compared over time. Secondly, the between-subject perspective, where two different entities are compared, ideally, at the same moment in time.

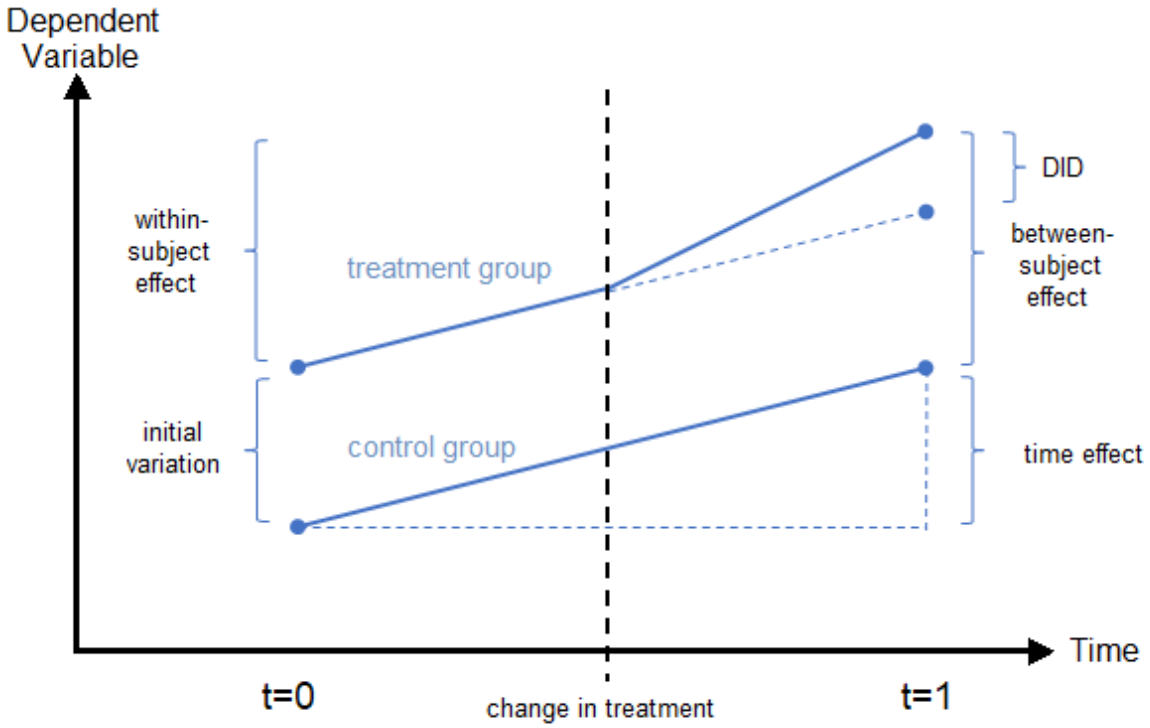
A drawback of pure within-subject comparisons is that a possible time-trend is not captured if there is no control group available. This could lead to falsely attributing any within-subject effect to changes in the treatment condition from the baseline to the next period. Pure between-subject comparisons, on the other hand, might falsely attribute treatment effects to initial variations in sample groups, a problem which can be particularly pronounced if the number of observations is rather low or if the randomization of the sample groups did not work properly.

One particular feature that I therefore introduce in my common-pool resource experiment, which builds the basis for the first two papers in this thesis, is a design that combines the advantages of within- and between-subject designs. I firstly have all participants start off with a baseline condition and only introduce the particular changes (treatments) in the second period. Additionally, I use a control group that is not subject to any change in conditions in any period. By doing so, I essentially create a panel design and consequently end up with data that exhibit both time and individual dimensions to compare.

Potential time trends that happen even without changes in the treatment condition can, hence, be controlled for and initial variations in treatment allocation do not go undetected, either. Based on this design, a difference-in-differences (DID) model can be calculated, which compares the differences from one period to another across treatment conditions. The following Figure (1)[3] illustrates the concept:

[3] Own illustration.

Figure 1: Difference-in-Differences concept[3]



The effect size according to the DID model can then be calculated as:

Equation (1):

DID(treatment) = within-subject effect(treatment) – time effect

Whereas the within-subject change of the control group is understood as the time effect.[4] The DID approach can be considered as particularly relevant for panel studies that include a time variable, e.g., comparing longitudinal developments across different entities using empirical real-world data, but also, as in our case, for experiments with repeated measures. Such can be both, repeated static or dynamic designs where the situation in the next round depends on what happened in the previous ones.

While the DID approach is less sensitive to either type of aforementioned errors, the required experimental design is slightly more complex. In the first paper of this dissertation, the one that analyzes effects of resource scarcity, all three types of comparisons, within-subject, between-subject and DID, lead to pretty much the same result. In the second paper, the one on the effect of external incentives and termination thereof, however, the advantages of the DID approach become evident. Within-subject or, even more so, between-subject comparisons only, would,

[4] The time effect is assumed as a common trend, which also affects the treatment group independently from the induced treatment condition.

in this case, lead to potentially wrong conclusions as the control group changes its behavior over time and also initial behavior is not entirely equal across all treatment groups. Both of these are fundamental problems that the DID can, fortunately, overcome. While I am surely not the first to use difference-in-differences methods, it has, up to date, rather sparsely been used in experimental economics.

The public good experiment as applied in the fourth paper, however, is constructed as a single-round (one-shot) game as we did, for this study, not intend to measure any dynamic or reciprocal effects.

Taking laboratory experiments into the field combines certain advantages (but also some disadvantages) from (natural) field experiments with those of laboratory experiments.[2] The advantages are a high level of control in the experiments and fast collection of data. Compared to lab experiments, we can expect a higher level of external validity as participants do not consist of homogeneous student samples, are not used to similar, previous experiments, and are usually sampled from a specific context that is relevant to the study topic. Field experiments might also be necessary for sampling participants that bring with them certain (e.g., socioeconomic) characteristics that the research design requires.[5] The particular aspects of the respective settings and their relevance to the research topics will be explained in the papers subsequently presented in this thesis.

3. Papers in this Thesis

Next, I will shortly present motivations for each of the studies in this dissertation and consolidate their relevance for the thesis' topic.

With the first paper, Prof Dr. Michael Kirk and I try to advance insights on the effects of resource scarcity. The allocation of scarce resources can be considered a very central and universal problem in economics, economic development as well as in welfare economics (cf. Smith 1776; Jevons 1879; Edgeworth 1881; Pareto 1906; Chenery and Kretschmer 1956; Sen 2001; Martin and Petersen 2019). The idea of this paper is to test, using behavioral experiments, if scarcity, in comparison to abundance, leads to higher or lower extraction by resource users. In this particular application we look at forests as a natural resource that can be converted into

[5] As disadvantages there is a more cumbersome data collection than in lab experiments and possibly a lower level of external validity than in non-experimental field experiments.

fresh farmland, but behavioral effects found in the study may be relevant for other types of scarce resources as well. The hypothesis here is that, even if a lower, sustainable rate of extraction is socially preferable, the prospect of the resource being exploited soon, might lead individuals to go and quickly secure excessive amounts of the resource. Investigating this could play a role in the use of natural resources, such as fish, land and forests; particularly considering future increases in demand and shortages in availability. Besides, it has also shown relevance very recently in consumer buying behavior regarding shelf-stable foods and bathroom tissue. While standard economic theory cannot predict any definite behavior, the experimental approach is able to isolate the effects and reveals that external resource scarcity, as modelled in our study, leads to reduced appropriation. The particular, novel contribution is that we do not compare levels of scarcity or levels of abundance, but actually create an abundant and a scarce environment in order to expose each condition's effect.

The next paper is also co-authored by Prof Dr. Michael Kirk and similarly builds on the assumption that individuals do not necessarily act entirely rationally. As a research question with proximity to real-world policy applications, we look at potential external solutions to social dilemmas in the form of introducing positive as well as negative monetary incentives. Discussion about whether rewards affect motivations to cooperate in more promising ways than penalties have long since been a discussion in academic discourse (Vollan 2008; Travers et al. 2011; Rode et al. 2015). Also, the comparison between individual and collective rewards can be relevant, not only from perspectives of feasibility but also and specifically as they might affect motivations to cooperate differently (Sommerville et al. 2010; Travers et al. 2011; Narloch et al. 2012; Gatiso et al. 2018). Such compensation payments for eco-friendly behavior, termed payments for ecosystem services, are an entirely realistic scenario and have already been applied in some settings (Wunder 2005; Pagiola and Platais 2006; Engel et al. 2008; Pattayanak et al. 2010; Vatn 2010; Engel 2016). It is well known that forests entail enormous positive externalities on a global scale that are, if measured in monetary terms, likely much more valuable than what local users can derive from them. It is therefore only logical to offer them compensation payments for conservation; a scheme that, for example, finds particular relevance in the United Nation's REDD+ program.[6] We do, once more, apply a common-pool resource field experiment with rural farmers as participants in order to test their behavioral response to three different types of external incentives. While all types of tested incentives will turn out as

[6] The full name of the REDD+ program is "Reducing emissions from deforestation and forest degradation and the role of conservation, sustainable management of forests and enhancement of forest carbon stocks in developing countries".

roughly equally effective, the particular addition that our study aims to make is to see what happens after such external incentives are terminated once again. Such a termination is just the logical consequence after the introduction, as any supportive or regulative policy measure can most likely not be sustained perpetually. The hypothesis is that there could possibly be adverse effects in the long run, in particular intrinsic motivations to cooperate or conserve might be permanently crowded out.

The third paper makes a small contribution on the theory of collective incentives. As a correspondence to an ongoing discussion, it was co-authored by my colleagues Prof. Dr. Björn Vollan and Dr. Tobias Vorlaufer (Gatiso et al. 2018, Salk and Travers 2018). We argue that the conditionality of collective incentives is based on collective performance of a group and therefore necessarily introduces, next to peer-effects, another level of a social dilemma to the respective situation. Free-riders might consequently benefit from collective incentives without making any effort towards the group target. It poses, thematically, a suitable addition to the second paper, in which, amongst others, collective vs. individual incentives are being compared.

Finally, I go into more abstract theory of individual decision-making behavior for my last paper. When looking for solutions to social dilemma problems, communication between affected individuals has been identified by a vast number of previous studies as a powerful measure to increase cooperation in groups (Dawes 1980; Sally 1995; Balliet 2010; Ostrom 2010). Little is known, however, about the exact mechanisms behind this, i.e., what happens in discussions that makes group members subsequently more likely to cooperate. Building on a newly developed theoretical framework and results from an unframed public good experiment[7], I can show the importance of both, social relationships between participants and the personal obligation of keeping promises that lead to the cooperation enhancing effect after communication.

4. The Data collection in the Field

For the purpose of collecting data for my studies I went to Namibia twice. Both research stays were conducted as part of the SASSCAL project and focused on the Kavango districts in the most northern part of the country that borders the Okavango river and, thereby, Angola. The region is the least developed in Namibia with the majority of inhabitants engaged in small-scale agriculture. There are large forests covering most of the area which are home to an abundant

[7] This study is actually based on just one half of the results from conducted experiment; the other part analyzes how anonymity and identification of group members affects cooperative behavior. Results here are also turn out as markedly interesting, albeit even more abstract and psychological.

flora and fauna. Further details on the region, its inhabitants and the sample of participants selected for the research will be presented within the following papers.

During my first trip, from June to August 2016, I conducted rather qualitative research in order to identify the relevance of different potential topics. In particular, headpersons from rural villages in the Kavango districts were visited and semi-structured interviews as well as open discussion questions were used to learn about problems of land tenure, forest use, agricultural practices and economic development. I also talked to many individual farmers, other local stakeholder as well as officials and workers from institutions and ministries. It turned out that one of our initially envisaged topics, land tenure security, could not be realized as the primary research objective. Albeit being a highly relevant topic, empirical data collection was impeded by reservations amongst the local population to freely discuss land tenure issues. Villagers perceive communal farming land and surrounding forest as their own and local, traditional authorities are not in favor of official registration of land on individual ownership due to the concern that doing so might consequently lead to sale of land and, thereby, loss of traditional land use rights.[8] Other issues were identified, amongst which there were elephants and wildfires destroying crops as well as deteriorating climate conditions affecting yields, but we eventually decided to focus on the problem of land conversion from forests into agriculturally used land. By doing so I followed previous researchers from our working group, who conducted field work on similar topics over the past decade, and I could partly build on their experiences and results. With a sharply growing population, the demand for land and loss of forest area that comes along with it, poses a highly relevant topic in the Kavango region that local farmers as well as traditional and official authorities are very much aware of. Disputes about land are not uncommon, migration from densely populated areas into more remote villages that still offer some available land are an issue and, more recently, farmers making claims on uncleared forest land in order to secure it for future use were found as increasingly frequent occurrences according to our qualitative findings. Based on our survey data (n=979), 27% of participants described available land as insufficient (26% by headpersons) and 30% stated that there has already been some rivalry or conflict in acquisition of new land (54% according to headpersons). Regarding the loss of forest, 70% confirm a rapidly decreasing forest in their village (73% by headpersons). We also asked the headpersons in each village in how many years they expect

[8] During the quantitative main research (n=979) we included one question to individual farmers as a survey item that asked about their perceived security of land tenure. 96% of participants answered that they felt safe on their land and did not worry about losing it. This was confirmed by our interviews with the headpersons, out of which 97% answered that land in their village was safe. Only 5% of headpersons stated that land in their villages was officially registered.

the remaining forest in their village to have entirely disappeared and got an average of 20 years as an answer.

Before finishing our first trip, we gathered permissions for large-scale, quantitative data collection from local authorities in preparation of the main research, which then started with my second trip to Namibia in March 2017. After assembling a local team of researchers, meticulous translation of experimental instructions and survey questions, pre-testing and calibration of experiments, as well as preparation of all required logistics, the main, quantitative data collection started in late April 2017. Overall, once the experiment started things went pretty smoothly, which I clearly see as a result of detailed and careful planning.[9,10] By early August, we had visited over 50 villages; more than 1000 villagers had participated in our experiments and answered our extensive survey questions. The exact procedural descriptions for each experiment and survey can be found in the respective papers and their appendices.

The collected data have since been used to create three of the following papers and results have been presented on several occasions, for example at the Land- and Poverty Conference at the World Bank in Washington, D.C., at the University of Tehran, at the Humboldt-University in Berlin and at other conferences. It has also served as the basis for numerous final theses written by students of various programs at the University of Marburg. Indeed, the dataset is much larger than what I could possibly analyze as part of my doctoral thesis and I will in the epilogue of this dissertation explain what other projects and papers can be produced with it. There were, especially, a lot of data collected from the individual surveys which allow studying topics that are not directly related to experiments and cooperation like the papers presented in this dissertation. These do, instead, focus on more applied aspects relevant to the region, such as agriculture, economic development, forestry, climate change and land use. If my future position

[9] It was particularly helpful to use tablets computers for both conducting the experiments and the survey interviews. They allowed quick data collection and direct upload into the dataset without wasting time or making errors by transcribing results from paper to computers. Further, results of the experiments could be automatically calculated and presented; consequently, no errors in payments for participants could be made. The instant availability of collected data allowed fast pre-analysis in the field, which saved us time and resources for pre-testing and calibration.

[10] As measurements of successful experimentation, a few indicators can be reported: Participants from the common-pool resource experiment have well understood the game (93%), perceived the payments as fair (63%) or more than expected (22%), and found the conducted game as relevant to real-world applications (73%). It was further stated by 98% that they would make similar decisions in real life as they made in the experiment, which is certainly a highly subjective measure, but a correlation between experimental and real clearing behavior is also confirmed by the data. The public good game was not quite as well-understood by our participants (81%), which might be a consequence of the more abstract, unframed nature of the instructions. Also in this game payments were perceived by the majority as fair (53%) or higher than expected (26%).

allows, I would very much enjoy completing these projects as I am convinced that there are still many interesting results hidden in there.

Before starting with the first paper, I will, on the following pages insert some photographs that give an impression of the research site in Kavango and the data collection in the field.[11] Photos were taken by Adrian Pourviseh, James Lukas and myself. Permission for publication was given by the creators as well as by all persons shown in the pictures.

5. Photographs

5.1 Kavango Region

Okavango River near Rundu

[11] Photos of experiments were taken with permission of participants and only during pre-tests and calibration. During the main data collection no photos were taken.

Forest with tar road

Pearl millet (mahangu) field

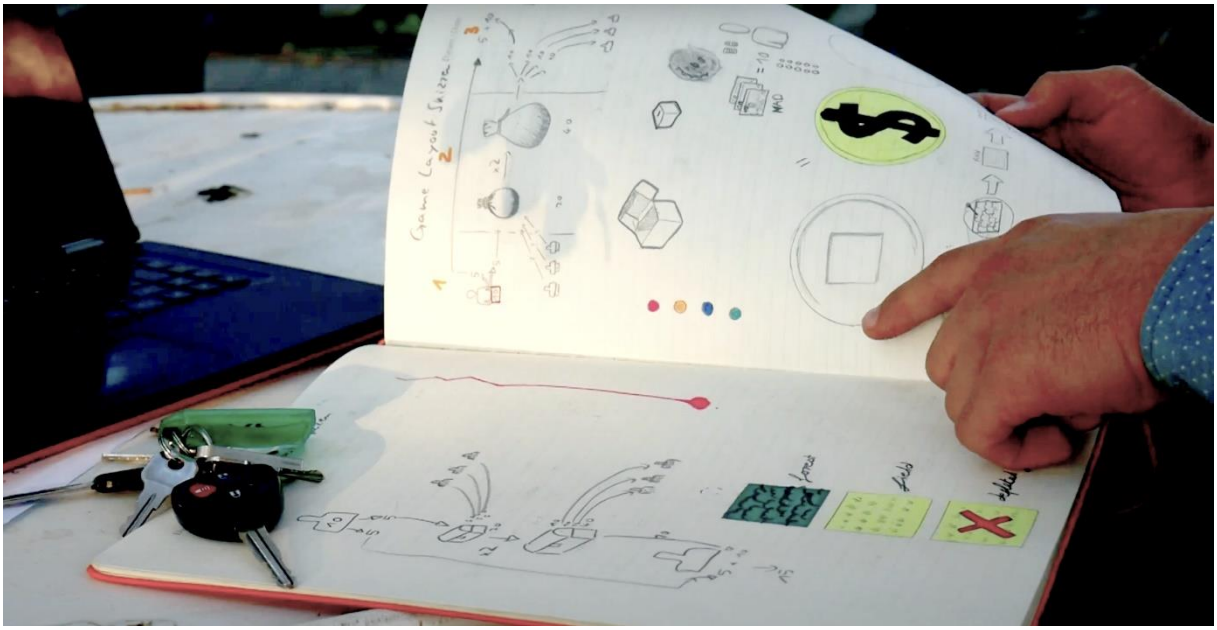
Livestock (cattle)

Gravel road with some fields near a village

Okavango river near Muveve

5.2 Experiments

Preparing experiments (1): Game design



Preparing experiments (2): Training

Team briefing before workshop

Drawing lots for random sample selection and treatment allocation

Public Good game instructions (1)

Public Good game instructions (2)

Semi-structured headperson interviews

Decision making with tablet computer (cardboard box to shield screen)

Survey interviews after experiments

Use of tablet computers

❖

References Preface

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Chapter I:

Does Competitive Scarcity Affect the Speed of Resource Extraction?

A Common-Pool Resource Field Experiment on Land Use in Northern Namibia

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JEL-classification: C91, C93, D91, Q15, Q23, Q24, Q5

Funding and Acknowledgements

Funded by the Southern African Science Service Center for Climate Change and Adaptive Land-Use Management (SASSCAL) through the German Federal Ministry for Education and Research [grant number 01LG1201B]. Special thanks go to all participating communities for their hospitality and to all research assistants, in particular Adrian, Anastasia, Blondy, Cypriaan, James, and Moses.

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[3] The authors declare that there is no conflict of financial, general, or institutional competing interests

Highlights

- Competitive vs. non-competitive appropriation conditions are compared
- Common-pool resource field experiment with real resource users in Namibia
- Participants make decisions in the experiment about clearing or conserving forest
- It turns out that resource scarcity slows down extraction rates
- Real-world experiences affect experimental decisions

Abstract

The aim of this study is to analyze how scarcity of resources affects at what rate users decide to extract or appropriate resources. We investigate this by conducting an economic lab-in-the-field experiment in northern Namibia. The participants in our study are small-scale farmers who regularly make decisions about either staying on their present fields or clearing forest areas for new ones. We compare environments where the stock of resources is abundant against environments where the stock of resources is scarce and extraction therefore competitive. Even though the experimental design allows keeping individual as well as social costs and benefits equal across both conditions, we find lower extraction rates in scarce than in abundant environments. Results also reveal that having experienced resource scarcity in the real world affects experimental decisions.

Keywords: common-pool resources, competition, field experiment, resource scarcity, land use practices, deforestation, conservation policy

1. Introduction

Scarcity of resources is a central problem in economics; natural resources in particular are expected to become increasingly scarce in the near future considering global economic development, a growing world population as well as growing per capita consumption and changing consumption styles (e.g., Schneider et al. 2011; Haddeland et al. 2014; Mancosu et al. 2015). In case of a resource stock becoming scarce, but not yet depleted, competition for what is left could increase and thereby extraction rates being sped up as users rush to secure remaining resources (Maldonado and Moreno Sánchez. 2009). If there is reason to conserve the resource, for example when a minimum stock is required for regrowth (like fishing grounds) or social benefits from the resource accrue (like collectively used forests or pastures), scarcity could, however, also stimulate a more considerate use of the remaining stock: the smaller it is, the less can be extracted without endangering the survival of the resource (Kramer 1989). In case of abundance, on the other hand, there is no competitive pressure, but also little reason to use the resource in a conserving manner. Hence, scarcity could, in theory, evoke different and opposing behavioral effects.

The aim of this study is to empirically examine the effects of scarcity in settings where a natural resource can be accessed by a group of users. We investigate this by conducting a lab-in-the-field experiment in order to gain insights about decision making behavior in common-pool resource situations. Local small-scale farmers in northern Namibia repeatedly have to make decisions about how often and how much forest area they clear in order to convert it into arable farmland. The particular advantage of studying forest resources is that they are immobile, large and openly visible; their current state can thus be assessed at any time and clearing activities can be observed by all users. Clearing decisions made by individual farmers have consequences for everyone in the community. If individuals take this into account in their actions, they show, as termed in the literature, social, or other-regarding preferences (Kagel and Roth 1995; Gsottbauer and Van den Bergh 2011. In a common-pool resource situation, non-selfish behavior, which leads to better outcomes for the group as a whole, is called cooperation (Ostrom 1990).

While the focus of this study is set on investigating behavioral responses to scarcity, it should be considered that, depending on the type of resource, scarcity can also have a direct non-behavioral effect on extraction rates: If increasing scarcity means that further appropriation requires higher effort, users might decrease their extraction due to simple economic considerations and stop entirely when extraction costs exceed benefits. Clearing of forests is a suitable type of resource use to study the behavioral response, as the individual costs and

benefits of clearing a hectare of forest do not depend on how much forest is left. In other words, a decline of forest area does not make it more difficult to find another parcel to clear until the stock is truly close to actual depletion. The analysis of forest resource use therefore allows measuring the isolated behavioral response to scarcity. As possible policy applications, results from this study might allow anticipating effects of increasing resource scarcity and help designing adequate policies in response. Also, it can be deduced if exogenous shocks in resource availability, such as new conservation policies or the introduction of protected areas, which limit the amount of usable, extractable resources, may have adverse effects on how remaining stocks are being used.

2. Background

The research was conducted in the Kavango region in northern Namibia (figure 1[1]). Unlike most of the southern, drier parts of Namibia, the Kavango area is a semi-arid zone with wet and dry seasons; large forests cover most of the region. Due to seasonal rainfall farmers are able to grow annual crops during the rainy season and harvest in austral autumn, after the rain has ceased. The vast majority of the rural population is engaged in agriculture with crop (millet) farming being the primary component of their livelihood and extensive cattle farming taking the second relevant role (Namibian Ministry of Lands and Resettlements 2015).

As the soil is mostly sandy, fertility decreases rapidly, and farmers leave their fields after few years to clear new ones in the adjacent forests (Mendelsohn 2009; Brown 2010). The freshly cleared forest area is then converted into fields, which are fertile and nutritious again and can be used for some years of cultivation. This practice is known as shifting cultivation[2] and together with area expansion for cropping it leads to degradation of forest in the region and a reduction of remaining, available land (Brown 2010). This has already become evident in densely populated areas along the Okavango River (figure 2[3]) where remaining land has recently become scarce and acquisition sometimes competitive (Pröpper et al. 2015). There have even been disputes between farmers over the acquisition of land for crop cultivation (Brown 2010). Further inland, however, forest stocks are still more abundant.

[1] Made with Natural Earth. Free vector and raster map data @ naturalearthdata.com.
[2] In the Kavango area, villagers do not usually entirely abandon the area and move their whole household and equipment to another area, but only move their fields further into the forest, often adjacent to their old ones. Cultivation can also be semi-permanent if farmers return to fields that had been left fallow in the past (Ruthenberg et al. 1980).
[3] Map source: google.com/maps, TerraMetrics map data, 2018.

Figure 1: The Kavango area in Namibia[1]

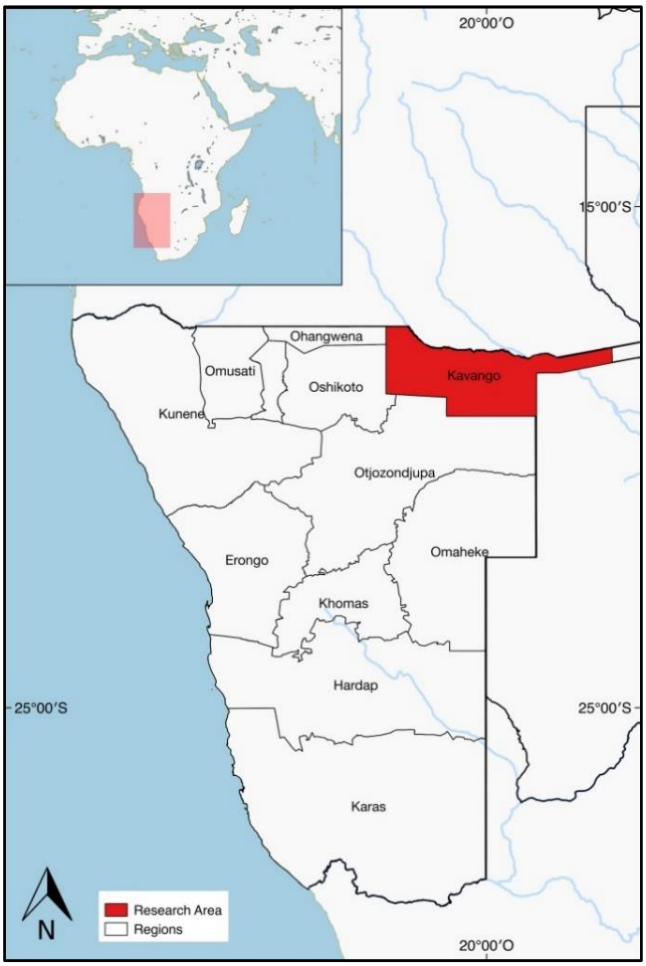


Figure 2: Satellite image of a part of the Kavango region[3]

Yet forests are not just available land for future agricultural use; they also constitute an important component of livelihoods for small-scale farming households and their communities. Those benefits provided by the environment and, in particular, forest areas are called ecosystem services (Costanza et al. 1997; Millenium Ecosystem Assessment 2005). For Kavango residents these can include the option of collecting resources like firewood, timber and foods (Flower and Rooyen 2001; Mamo et al. 2007; Barnes et al. 2010; Luckert and Campbell 2012). Surrounding forests become particularly important in the slack season before the harvest. Firstly, because they provide an additional source of food[4] when grain stocks from the previous year's harvest are depleted (Flower and Rooyen 2001). Secondly, because cattle herds are sent into the forest for grazing to avoid harming the growing crops during that time (Flower and Rooyen 2001). Indeed, it is estimated that a household's direct yield value from the surrounding forests is much larger than the crop value from their individual fields[5,6] (Pröpper et al. 2015). One hectare of recently cleared field, however, produces higher yields than a single hectare of forest. Therefore, clearing forest and cultivating new fields is individually more profitable in the short term (Pröpper et al. 2015). Due to climate and soil conditions in Kavango, regrowth of forest on fields that were left fallow again is marginal and almost negligible as it takes centuries to regrow to its previous state (Immanuel 2019).

Our qualitative interviews and quantitative surveys, which were conducted as a part of the research project, confirm that local authorities and villagers are aware of unsustainable clearing practices being a problem in the long run and for future generations. As explained above, forests provide a multitude of benefits to the communities.[7] We therefore interpret the forest situation as a social dilemma, which develops over time. Clearing some hectares of forest does not cause a big loss at that moment or even year, but after some years of clearing, the damage done by deforestation accumulates[8]. This circumstance will also be reflected in the design of the

[4] According to survey results on forest use that we collected as part of this study, these include fruits, mushrooms, roots, herbs and small game.

[5] See also Angelsen et al. (2014) and Wunder et al. (2014) on the role and importance of livelihoods that depend on forests.

[6] In our study sample (n=252), 71% of participants stated that they use forest as grazing land for their cattle, 83% for collecting firewood, 90% for collecting timber, 90% for collecting fruits and other foods, and 40% even use the forest for monetary profit such as selling forest products.

[7] On a larger scale, forests reduce the danger of desertification in dry areas, prevent land degradation like erosion of soils and further offer habitats for many species, hence preserving biodiversity (Costanza et al. 1997; Daily 1997; Grainger et al. 2000; Geist and Lambin 2004; Engel et al. 2008; Grainger et al. 2009). Globally, they contribute to carbon storage and sequestration; thereby playing a crucial role in combating climate change (Gullison et al. 2007; Pan et al. 2011, Houghton et al. 2015; Griscom et al. 2017, Mitchard 2018). Slower rates of shifting and clearing would therefore not only bring about benefits to the local communities and allow more sustainable long-term land use but also contribute to a globally healthier environment.

[8] Note that clearing should in the local context not be considered as bad behavior as agriculture constitutes a major part of livelihood for most people in the area and poverty might not leave scope for deliberate reductions of consumption in order to conserve environmental resources.

conducted common-pool resource game, where social payoffs do not happen continuously but at the end of the game. Similarly, the time lag between direct private yields and social benefits that occur later incorporates an uncertainty as villagers do not know how much of the resource will be left by the other users in the long run (Kramer 1989). This uncertainty is particularly pronounced when resources are scarce.

Land in the Kavangos, forests as well as fields for cultivation, is mostly not privately owned by the people who use it. Formally, it is communal land owned by the state but traditional authorities, such as village headpersons, exercise some degree of control over the land (Mendelsohn 2009). They do, for example, make decisions about granting land to migrants who wish to settle in their area. In many villages there are some rules established about clearing forest. If so, the headperson usually has to be asked for permission to clear, but clearing is seldomly denied to local villagers[9] (Mendelsohn 2009). Mendelsohn (2009) already points out that the existing communal tenure system provokes individual interests to exploit the commonage resource to the maximum. Locally, within the village, land that has been cleared and cultivated is normally considered property of the respective farmer[10]. However, without formal, private land rights and titles there are little incentives for responsible, sustainable land use, little long-term investments into land and no possibility to use it as collateral (Mendelsohn 2009; Namibian Ministry of Lands and Resettlements 2015).

3. State of the art response on scarcity and development of hypothesis

To our knowledge, only few empirical studies have paid interest to how the size of a resource stock affects its extraction and survival. Essential work on natural resource use and ecosystems that are managed by groups was conceptualized by E. Ostrom in the 1990s (e.g., Ostrom et al. 1994; Ostrom et al. 1999; Ostrom 1999). E. Ostrom also formalized a framework of important variables for analyzing socioecological systems, which also includes the size of the resource system (Ostrom 2009). Some marketing and psychological studies (e.g., Brock 1968; Verhallen 1982; Lynn 1989; Lynn 1991; Verhallen and Robben 1994; Gierl and Huettl 2010; Aguirre-Rodriguez 2013) have discussed which responses scarcity of goods and products could provoke but according to their findings, no clear picture could be drawn but the results depend on the respective situations and circumstances. One particular result that crystallizes from various

[9] In our sample, 10.9% stated that they have been denied clearing at least once in the last years.

[10] Sometimes farmers can make claims on forest parcels they intend to clear in the future. These are often adjacent to their old field. Forest areas that have not yet been cleared do, however, in general not belong to individual villagers.

findings is that scarcity as a consequence of high demand raises a product's attractiveness for potential consumers whereas scarcity due to low supply does not have the same effect.

Uphoff et al. (1990) suggest based on findings from irrigation system management in Sri Lanka that the relationship between scarcity and the degree of cooperation could be non-linear: Cooperation is highest when the resource is somewhat scarce, i.e., neither very scarce nor abundant. Similarly, Araral (2009) found that water scarcity in irrigation associations in the Philippines has a curvilinear effect on collective action. Bardhan (2000) investigated another irrigation environment in India and also finds that extreme scarcity has a negative effect on cooperation. Also, Varghese et al. (2013) found resources scarcity as the cause for less efficient usage. Where real world observational data are hard to obtain, scientific insights about decision making in social dilemma situations have been established and extended with the help of economic experiments. Lab and, in the last two decades, lab-in-the-field experiments have increasingly been applied as they allow isolating (behavioral) effects and identifying factors that cannot be explained by standard economic theory (Kagel and Roth 1995; Harrison and List 2004). An early experimental study on the effects of scarcity was conducted by Rutte et al. (1987). They found in a common-pool resource experiment with university students as participants and sequential decisions on extraction that more resources were extracted in an abundant environment. A distinction was made between exogenous (environmental condition) and endogenous (player-induced) scarcity with the result that the differences in extraction rates between the scarcity and the abundance condition were greater in the exogenous condition. Similarly, Osés-Eraso and Viladrich-Grau (2007) and Osés-Eraso et al. (2008) examined appropriation rates in common-pool resource situations in other classroom experiments. Their findings also suggest that initial, exogenous scarcity reduced appropriation rates. Endogenously increasing scarcity that is caused by the subjects' previous decisions also reduces appropriation rates, albeit to a lesser degree so that depletion cannot be prevented in the long run.

Gatiso et al. (2015), however, found contrasting results in a forestry-framed field experiment: A scarce environment led to higher extraction rates than an abundant one in their common-pool resource game. In their design, lower, sustainable extraction was socially optimal because of constant regrowth rates of resource stocks. Higher total harvests could be achieved if players initially restrained their extraction. Their findings are also in line with Blanco et al. (2015) who conducted a common-pool resource experiment in Colombia that was framed as water extraction. They compared different levels of reductions in resource availability and found that in the scarcest environment, appropriation rates were highest. Kramer (1989) further noted that conflict might arise within groups due to scarcity if individuals do not cooperate but try to

maximize their own share. Prediger et al. (2014) even find that participants, who have experienced exogenously given resource scarcity in their livelihood, act more antisocially in a field experimental setting.

An important distinction that several of the aforementioned authors make is between exogenous, as environmentally-given, and endogenous scarcity as the consequence of the very same resource users' behavior. It becomes intuitively clear why the difference is crucial: By unsustainably extracting resources, users reveal themselves as selfish, leading other users, even those who would have been willing to restrain from excessive extraction, to subsequently also act selfishly and try to secure some resources for themselves before they are taken by others. If any conversation effort to spare resources can, in the end, not be expected to result in any social benefit, it clearly becomes an entirely unattractive, even unreasonable strategy. Likewise, Kramer (1989) concludes that if scarcity is a consequence of failed cooperation, then users will most probably respond by not cooperating either (cf. Osés-Eraso et al. 2008). Exogenously given resource scarcity is, on the other hand, not a consequence of its users' behavior. Users might, due to the challenging conditions, even be more inclined to realize the need for cooperation and careful extraction behavior (Gibson et al. 2007). In the present context, it might at first seem unclear if scarcity of forests in Kavango should be considered exogenous or endogenous. The differences in forest availability between the hinterland, which has only been populated in the last decades, and the populous riverside are not directly attributable to individual behavior of farmers but must be seen as a consequence of decades of clearing by a higher number of people living at the Okavango river (Mendelsohn 2009).[11] For this study we hence understand existing scarcity as an external circumstance. At the same time further deforestation should not be considered as inevitable but as a long-term consequence of unsustainable individual decision making. This is also reflected in the design of the experiment in which scarcity is externally introduced as a treatment condition. Due to the dynamics of the game, which goes over several rounds, there is, however, not only externally given scarcity, but it can additionally worsen by decisions made by players, i.e., endogenously.

[11] This should not obscure the fact that due to currently high population growth and potential changes in ways of life as well as agricultural habits and methods deforestation is now more than ever becoming an urgent challenge in the Kavangos.

The novel contribution of this study now consists of three factors and their combination:

(1) The setting into the field context in northern Namibia with small-scale farmers as participants who make similar decision in their real lives. There are villagers participating who live in resource-abundant and villagers who live in increasingly scarce areas, which will be considered in the analysis.

(2) Introducing an experimental "scarcity" environment and comparing it with an "abundance" one. We do not compare levels of scarcity (or levels of abundance, respectively) against each other, but actually model one competitive environment where resources could be depleted against an uncompetitive one where resources could not be depleted.

(3) Employing a newly designed, dynamic common-pool resource game where social benefits of the forest accrue at the end, which represents well the situation in the given environment, as it incorporates the uncertainty and the temporal distance between short-term private and long-term social payoffs.[12]

The hypothesis to be tested in this experimental study is formulated as:

> ***H0:*** *There is no difference in extraction rates from a common-pool resource in a scarce and in an abundant environment*
>
> ***H1:*** *In a scarce environment, extraction rates from a common-pool resource are different from those in an abundant environment*

The hypothesis is kept neutral as, ex-ante no particular direction of a potential effect is anticipated. The experimental approach is meant to identify behavioral patterns and isolate the potential effects induced by scarcity from other non-observable influences that could be present in real-world environments.

[12] Of course, as all payoffs of the game were done on the same day, there is no substantial delay between private and social benefits, but since the social benefits only happen at the end of each game, there is insecurity about how much forest will be left by the end. This characteristic would not be as distinct if social benefits occurred in every round.

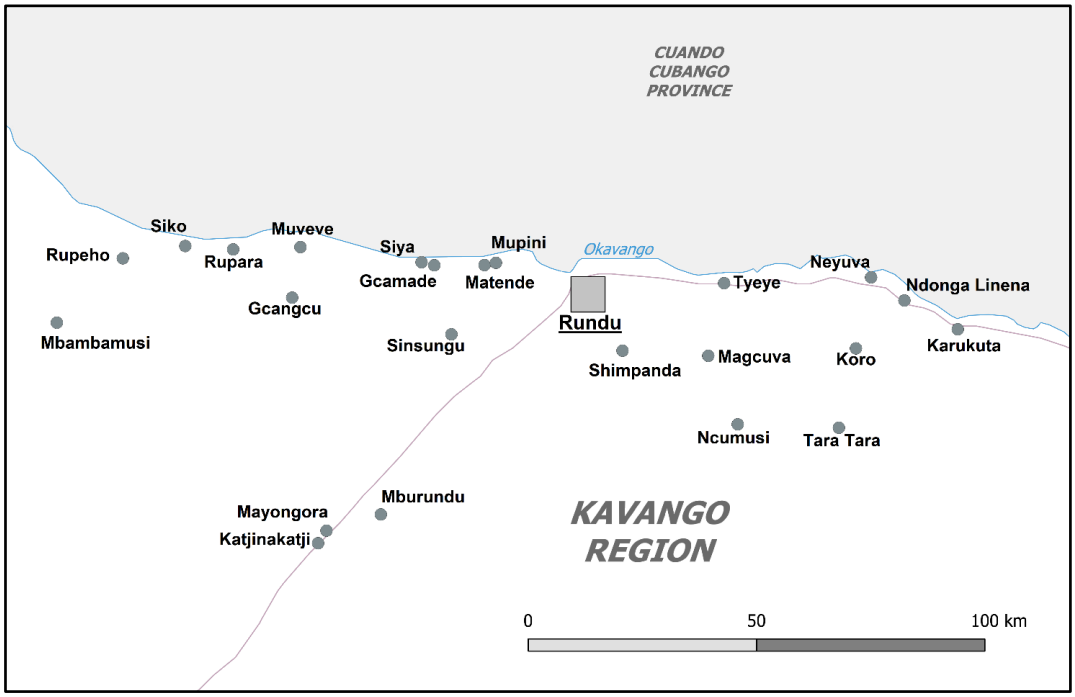
4. Method

In this chapter the empirical approach of this research will be explained, starting with information on the participants, followed by the experimental procedure, the common-pool resource game design and, finally, the method of analysis.

4.1 Sample

In winter 2016 (June – August) explorative interviews and surveys were conducted with Namibian officials, workers from development institutions, traditional authorities and with local farmers in the Kavango region in preparation of the main research. In winter 2017 (May – August) the individual surveys and experiments were conducted in 23 randomly selected rural villages (figure 3[13]) in two Kavango West districts (Kahenge and Kapako) and two Kavango East districts (Mashare and Ndiyona).

Figure 3: Villages visited in the Kavango area[13]



Some villages were left out in the sampling due to their proximity to the town of Rundu, where a substantial share of inhabitants is engaged in activities other than agriculture. Preconditions were also that the village had more than 80 inhabitants and was not more than a day's drive away from the nearest tar road. Villages were randomly sampled from both the more densely

[13] Map made with QGIS: "QGIS Development Team (2020). QGIS Geographic Information System. Open Source Geospatial Foundation Project. http://qgis.osgeo.org"

populated riverside and the inland regions where forests are still more abundant. Figure 3 shows a map of the selected villages' locations in the Kavango area[14].

The total sample of this experiment consists of 252 participants, 126 in each of the two treatments. [15] Socioeconomic characteristics of the sample and a description of variables used for regression models are shown in table 1. Split samples for the two experimental treatments can be found in table 2.[16]

Table 1: Variable and sample overview

variable name	mean	sd	min	max	info
Age	39.41	14.47	18	76	age of participant (min. 18)
Female (d)	0.61	binary	0	1	gender (1 for female)
Schooling_years	6.10	4.06	0	19	years of schooling
Adults_in_hh	3.61	2.57	0	23	number of adults in household
Village_native (d)	0.61	binary	0	1	born in this village
Bags_farming_yield	10.62	16.40	0	189	bags of farming yield produced last year
Hectares_cultivated	2.83	2.60	0	20	hectares currently in use for cultivation
Hectares_cleared	1.72	2.30	0	21	hectares cleared in last clearing session
Clearing_rules (d)	0.52	binary	0	1	rules about clearing present in village
Land_rivalry (d)	0.28	binary	0	1	land acquisition perceived as rivalrous
River (d)	0.46	binary	0	1	village located at river (real scarcity)
Observations	227				

Note: values for cleaned dataset
Source: own calculations based on collected data

4.2 Procedure

We visited each village's headperson several days before the experiments in order to arrange an appointment for a village meeting so that all villagers could be informed and invited. It was made clear beforehand that some monetary compensation would be offered for participating but also that only a certain number of people could take part in the workshops. At the beginning of the village meeting, participants were randomly drawn by lot amongst those willing to participate, a process which was mostly perceived as fair by everyone. The same lots also determined the allocation of experimental treatment groups. The experimenters, together with local, trained research assistants then explained the procedure and the instructions of the common-pool resource game, also by showing posters and giving examples for different

[14] The Kavango is administratively divided into Kavango West and Kavango East since 2013. Rundu is the second largest city of Namibia and the rapidly developing capital of Kavango East.
[15] The total sample size of the data collection was 979, all of which took part in economic experiments, but the remaining 727 participants played different games or treatments that were related to another research. These observations and results are therefore not reported here.
[16] After data cleaning there are 227 valid and usable ones left. All tables, results and graphs in this paper will be based on the cleaned dataset of 227 participants.

decisions but without valuing or recommending any particular behavior.[17] Special attention was being paid to making clear that the game was not a "zero sum" situation about dividing the yields, but that cooperating actually increases the total benefits for the group as a whole.

Before the game was played and the particular treatment instructions were read, treatment groups were spatially divided to ensure that other groups' decisions or their treatment instructions could not influence the outcome. Tests for understanding were carried out with the group and individually. Our assistants also gave additional help and instructions to those who did not understand everything right away and to those who needed help with using the tablet computers that were used for decision making. We did, however, make sure that everybody was fit for the decision making in the real game and did not require assistance once the game started. Hence, all game decisions could be made by the players individually and anonymously. Before the game started, one trial round was played, yet the results from this trial round were not made public.

After finishing the common-pool resource game, each player was asked two control questions for understanding of the game mechanics.[18] If one or both of the control questions were answered wrongly, the player would still receive their payments but the respective observation is not considered in the analysis.[19]

Then, after a short break, individual surveys were conducted with each player.[20] Payments according the participant's and their group members' decisions were done in the very end, individually and in private. The whole workshop took about 4-5 hours in each village. Payoffs averaged at 78N$ (~6US$) per participant, which is more than an average local daily wage. We also provided snacks and cool drinks to all participants.

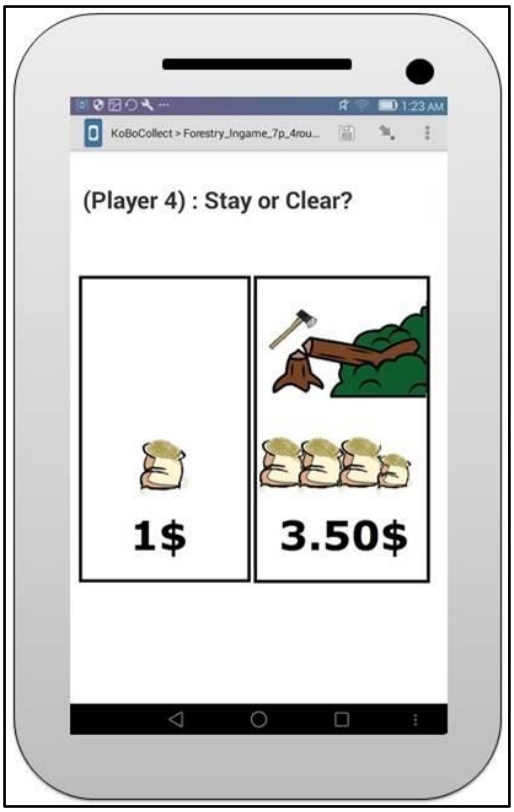
4.3 The common-pool resource game

For the experiment, a dynamic forestry-framed common-pool resource game was designed that resembled decision making about clearing new fields in real life. Participants could earn real money according to their own and their group members' decisions. The game was specifically

[17] Protocols and instructions were translated by our assistants from English into the respective local languages and then translated back into English by another assistant in order to ensure that all translated instruction were on point. Also, all wordings and phrases used in the instructions were discussed intensively with our local assistants in preparation of the experiment as to make all instructions clear and easily understandable. Protocols for village meeting and game instructions can be found in the supplementary materials (C).
[18] The game was correctly understood by the majority (90%) of participants.
[19] Because of this, sample sizes are slightly different between the two experimental treatment groups.
[20] Survey questions can be found in the supplementary materials (D).

designed to match the given conditions in the study area and to allow experimentally testing abundance against scarcity conditions[21].

Figure 4: Tablet computer screen for making decision about staying or clearing[22]



There were always 7 players in a group playing the game together. In each of four rounds played, participants had to decide privately and anonymously whether to clear a new field or to stay on their old one (figure 4[22]). No communication between players was allowed. The socially optimal outcome was reached when everyone decided to stay on their old field only. Staying meant getting a smaller private yield of one bag, represented by 1N$, from the old field. Individually, one could earn more from clearing new fields as a fresh field yielded three and a half bags, represented by 3,50N$ in the game. It was therefore individually optimal to always clear (Nash equilibrium). Each clearing of a new field did, however, decrease the size of the forest by one parcel and after each round, it was announced how many parcels had been cleared

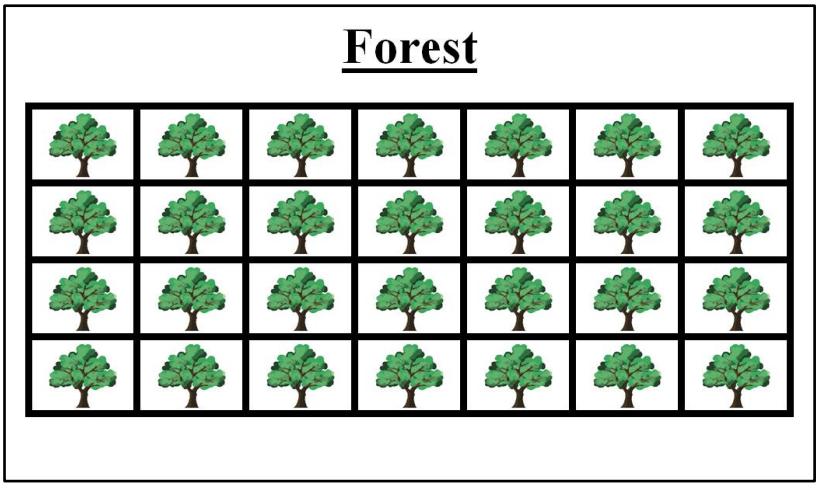
[21] It was therefore necessary to deviate from the established forestry-framed common-pool resource game as described by Cardenas et al. (2013) and Janssen et al. (2013) in favor of a design where social benefits accrue from the forest left at the end and not from regrowth rates of forest. Firstly, regrowth of forest in the Kavangos is marginal due to climatic and soil conditions. Secondly, social benefit generation based on regrowth rate of the resource could imply that a final depletion of the stock is socially optimal, which is not a desirable outcome in our context. Thirdly, a design where social benefits of conservation originate from regrowth rates does allow comparing different levels of resource stock (e.g., Gatiso et al. 2015), but not a really abundant environment.

[22] Kobo-Toolbox was used for data collection on tablet computers during the experiment as well as for the surveys. https://www.kobotoolbox.org/ Figure own illustration based on Kobo-Toolbox layout.

in that round by the group as a whole. Forest parcels, represented by trees in a poster (figure 5[23]) were then accordingly crossed out so that all players could asses the current stock of the forest at any time, but it was never revealed who had cleared and who had not. At the end of the game, all players received their benefit from the forest which equals the number of parcels left, i.e., 1N$ per parcel of forest for each player.[24,25] The whole game was kept simple by asking players to decide only between the two options "staying" and "clearing" in each round. The accumulated private yields as well as the social benefits that occur at the end added up linearly.[26] Earnings were represented with the real Namibian Dollars as paid after the game[27] and decisions were made with tablet computers where the two options were additionally represented by simple pictures (figure 4).

Figure 5 illustrates the "abundance" stock of 28 parcels of forest, as played in the first period by all players and in the second period by the control group playing the "abundance" treatment for a second time. Due to the total forest stock in the abundance treatment consisting of 28 parcels of trees, there was no competition in resource extraction: with 7 players per group and the game being played over four rounds, the maximum that could be extracted if all players always clear is 4 x 7 = 28 parcels.

Figure 5: Poster showing 28 parcels of forest[23]



[23] Own illustration.
[24] Even though some households also sell goods that they collect from the forest, most of the forest goods are for self-consumption and cannot easily be monetarized or the total benefit's value be estimated. One Namibian Dollar payoff for each parcel was therefore chosen as an appropriately calibrated amount for the common-pool resource game and for general simplicity.
[25] The game design does not incorporate the possibility of non-linear social benefits of forest size or a threshold level, at which the forest provides a maximum of social benefits.
[26] A more complex game design that incorporated yields decreasing non-linearly with the age of the fields, was dismissed due to comprehension difficulties after pre-testing in the field.
[27] Payoffs for staying and clearing were set after intensive pre-testing for calibration and allowing a reasonable final compensation for participating in the workshop. For simplicity, we renounced the possibility of introducing a game currency that must then be converted into real money.

Consequently, no player had to worry about not being able to clear anymore towards the end of the game. This case was, amongst others, also shown in an example calculation during the instructions, in order to emphasize non-scarcity in the baseline condition.

Figure 6: Game and treatment design[28]

Period	Period 1 "Baseline"	Period 2 "Treatment"
Round	Round 1 2 3 4	Round 1 2 3 4
Control Group "Abundance"	Abundance Condition (28)	Abundance Condition (28)
Treatment Group "Scarcity"	Abundance Condition (28)	Scarcity Condition (14)

The sequence of rounds and treatments is illustrated in figure 6[28]. After playing the abundance treatment for the first period the game restarted. In the second period the control (abundance) groups played the same game again whereas the scarcity treatment groups then faced a reduced resource stock of 14 parcels of forest. Another poster, which looks similar to the one in Figure 5 but shows only 14 trees, was used for the scarcity treatment. By using this treatment design, scarcity effects can be analyzed from two perspectives: Firstly, as a within-subject design where the same player's change in behavior from "abundance" in period 1 to "scarcity" in period 2 is analyzed. Secondly, as a between-subject design where players' behavior in the "scarcity" environment of the second period is compared to the control group, who played another "abundance" environment in the second period. Building on both, a difference-in-differences model can be calculated, in which the changes in clearing behavior from period 1 to period 2 are compared across the two experimental treatments.[29]

[28] Own illustration.
[29] An additional reason for firstly applying an "abundance" baseline for all treatment groups instead of directly starting with the scarcity environment was to indirectly point out the existence of an abundant and a scarce condition.

4.4 The scarcity condition

The distinctive characteristic of the scarcity condition was that appropriation of resources was competitive: With the reduced "scarcity" stock of 14 parcels, it was possible that the forest gets depleted before the end of the game. In the extreme case of all 7 players always clearing, this could happen by the end of the second round. Hence, even if a player or a group of players restrained themselves from excessive clearing, the forest stock could easily be depleted by the other users in the scarce environment. While in the abundance treatment each spared parcel of forest led to a safe social benefit, in the scarcity treatment there was competition for resources and uncertainty about whether conservation efforts actually resulted in any social benefits in the end. If there was no forest left, no more clearing was possible but all players received a yield for staying of 1N$ for the remaining rounds of the game. Before playing round 3 and round 4 it could also happen that there were less than 7 parcels of forest left. If in those rounds more players decided to clear than there were parcels left, the yield for clearing was reduced from 3.50N$ to 2N$. We set this as a simple rule in order to keep understanding facile and to avoid calculating with fractional numbers.

Receiving a reduced clearing yield of 2N$, players should have been theoretically indifferent between clearing and staying as 1N$ from staying plus 1N$ from social benefits that accrued from the spared parcel of forest in the end summed up to the 2N$ that could alternatively be earned right away by clearing. This consideration does however not hold for two reasons: Firstly, no player knew what the other players in their group were going to decide in the respective round. Therefore, there was still the possibility of getting 3,50N$ and thereby increasing one's payoff by clearing in case the numbers of players clearing did not exceed the numbers of forest parcels left. And secondly, as indicated before, even if the expected gain from clearing was only 2N$, it could happen that the rest of the forest was just taken by the other players, leaving no social benefits for anyone in the end. In this case, 2 N$ was still preferable to 1$ from staying and clearing hence remained the individually best option at all times. The scarcity treatment did therefore not change the individually or socially optimal decisions in the game compared to the abundance environment.[30,31]

[30] In order to avoid last-round effects, players were not told in advance how many periods, i.e., repetitions of the game they were going to play in total There was indeed a third period of the game played after the end of period 2, but observations from this third period are part of another study, and therefore not discussed here.
[31] A more detailed treatment description, including formalization of payoff equations can be found in the supplementary materials (B).

4.5 Method of analysis

The development of clearing behavior in all rounds and treatments is firstly presented as a graph and complemented by a table that includes the average clearing in each round plus the corresponding standard deviations. The inferential analysis then consists of three components: Firstly, a within-subject comparison, where changes in clearing behavior from period 1 to period 2 are analyzed. Secondly, clearing rates are compared between-subjects in the second period. Finally, panel regression models are presented that include all periods and treatments at the same time. Here, changes in clearing from period 1 to period 2 can be compared between-treatments, which equals a difference-in-differences approach (DID). Treatment effects are tested with simple Wilcoxon-Signed-Rank and Mann-Whitney-U tests, followed by multivariate regression analyses. Clearing outcomes as the dependent variable of interest are always presented on round level in all regression models.[32,33] Despite the binary nature of our dependent variable, the decision to clear or not to clear, we give preference to a linear probability model (LPM) over logistic regression models due to better interpretability of coefficients, in particular of independent dichotomous variables, such as the effect of the scarcity condition. Since the experiment was well calibrated, the average outcome is close to 0.5 (on a scale from 0 to 1), which supports the use of the LPM model. In the panel data analysis, we apply both fixed and random effects models. The random effects models require any potentially omitted variable to be uncorrelated with the explanatory variables, which should ideally be the case due to randomized treatment allocation. The random effects approach further allows the inclusion of time-invariant explanatories, which are also of interest, such as the presence of clearing rules and the location of the village. Fixed effects models are additionally presented to compare changes within individuals only. They ignore any time-invariant control variables and are necessarily consistent, albeit potentially less efficient (Halaby 2004; Clark and Linzer 2015; Bell et al. 2019).[34] Both types of panel regressions firstly include dichotomous

[32] An aggregate clearing per period (over all four game rounds) may not be an adequately precise measure in the scarcity condition: Once all 14 parcels of forest are cleared, which can happen after 2 rounds of playing the game in the scarcity condition, no more resources can be extracted. Sheer non-availability could then affect clearing outcomes with the danger of being falsely attributed to resource-conserving behavior on the aggregate level. However, it turned out that even the scarce stock was never actually completely exhausted before the last round was played. Endogenous and reciprocity effects as well as reduced payoffs from clearing in later rounds might nevertheless affect decisions.

[33] For this study we are primarily interested in the effect of resource scarcity on a large scale. Clearing outcomes are therefore always compared in absolute numbers and are never set in relation to the size of the stock.

[34] While, in theory, the randomized treatment allocation should allow the use of the random effects model, we decide to report both, results from random- as well as fixed effects estimations. We wish to keep the cleaner interpretability of fixed effects coefficients, but also make use of the random effects models' capability of including time-invariant exogenous variable, which are of interest. It will later turn out that coefficients for the designated treatment conditions are almost identical for both methods. In that way, both models also serve as an additional robustness test against each other.

explanatory variables for both the scarcity as well as the control treatment rounds. In a next step, the control treatment effect is understood as the general trend and used to calculate the difference-in-differences model.[35] It is hence subtracted from the treatment effect of the scarcity condition, resulting in DID-coefficients. Comparisons over time are always against the average clearing of period 1 as it cannot be assumed that a certain pattern repeats itself in each period (which would allow comparing the respective single rounds against each other e.g., the first round of period 1 against the first round of period 2). Between-subject analyses as well as between-subject variation in random effects models, however, base on round-wise comparisons. For the multivariate regression models, standard socioeconomic characteristics are included as well as a set of indicators for real-world clearing behavior and land use. While the actual location of the village is represented by the "River" variable, "Land_rivalry" captures whether the particular individual perceives acquisition of new land as competitive according to their survey answer. Further, rules about clearing forest that have been established in the village may affect clearing behavior in the experiment and are therefore added to the model.[36] When separating the sample into the two treatment groups, it becomes evident that not all socioeconomic variables are evenly distributed over the two treatment conditions (table 2). In order to avoid confusing the treatment effect with potentially influential initial difference in treatment allocation, variables described in table 2 are included in the regression models.

Table 2: Sample split over experimental treatments and differences

	abundance		**scarcity**			
	mean	sd	mean	sd	difference	p
Age	40.85	14.92	38.00	13.93	2.84	0.185
Female (d)	0.65	binary	0.57	binary	0.08	0.276
Schooling_years	5.63	4.22	6.56	3.85	-0.92*	0.074
Adults_in_hh	3.96	2.88	3.28	2.18	0.68**	0.028
Village_native (d)	0.60	binary	0.63	binary	-0.03	0.685
Bags_farming_yield	11.42	12.91	9.84	19.22	1.58***	0.006
Hectares_cultivated	2.71	1.66	2.95	3.26	-0.23*	0.063
Hectares_cleared	1.53	1.52	1.91	2.86	-0.38	0.766
Clearing_rules (d)	0.58	binary	0.47	binary	0.11	0.111
Land_rivalry (d)	0.30	binary	0.25	binary	0.05	0.459
River (d)	0.39	0.49	0.52	0.50	-0.13*	0.062
Observations	112		115		227	

P-values for Mann-Whitney U-test or Fisher's Exact-test in case variable value is binary
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculations based on collected data

[35] By doing so the effect of the control treatment rounds is absorbed in the time variables and therefore no longer included.

[36] The dummy variable for the presence of clearing rules was collected on individual and not on village level. There were some cases where participants from the same village made different statements about clearing rules in their village, but for the analysis the variable is kept on individual level and not corrected for village level, as the single participant's perception about the clearing rules is assumed to be the relevant factor.

After presentation of the main models, we will also look into endogenous effects, in particular, if scarcity caused or aggravated endogenously by a group's excessive clearing behavior affects decisions in later rounds. For this purpose, an additional table reports outcomes individually for both treatments and periods and includes the number of trees that have been cut by the other group members up to that round as an explanatory variable (table 5). Also, a variable for a player's own clearing behavior up to the respective round is added in the model in order to distinguish between endogenous scarcity caused by the other players and a reduced stock caused by one's own previous clearing.

Within-subject and between-subject regression models, including the fixed effects model are based on ordinary least squares (OLS) estimations, while the random effects model uses the generalized least squares (GLS) estimator by default. All reported standard errors are clustered on individual level. Complementary tables for a within-subject comparison of the control group as well as a between-subject comparison for the first period can be found in the supplementary materials (A). R-squared measures and F-tests are reported right in the respective regression tables. Further, tests for multicollinearity were conducted for selection of control variables in order to avoid problems with correlated independent variables. Due to the nature of the LPM models, tests for homoscedasticity and distribution of residuals are not reported.

5. Results

Figure 7[37] shows the average clearing outcomes for each round and treatment condition. Corresponding values and respective standard deviations can be found in table 3. In the table, differences within- and between-subjects are shown complemented by appropriate tests.

It turns out that there is little initial difference in average clearing between the two treatment groups in the first period, when all players were confronted with the abundance environment. There are some variations within the single rounds, but the final, aggregate as well as the average outcomes are almost identical for both treatments: An average of 0.49 parcels were cleared in abundance control groups and 0.47 parcels in the groups that were later going to be confronted with the scarcity environment (table 3). The difference is not significant according to a Mann-Whitney U-test (table 3).

[37] Own illustration based on collected data, created with Stata 15 statistical software.

Figure 7: Average clearing over rounds by treatment group[37]

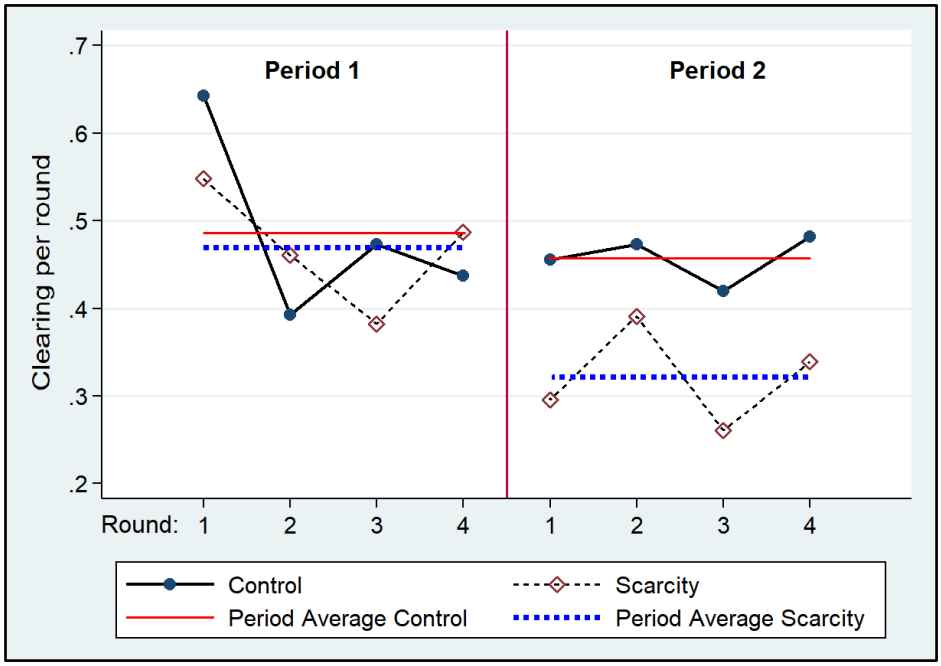


Table 3: Average clearing in each round

Abundance Groups (n=112)	**Period 1** mean	sd	**Period 2** mean	sd	Within-subject difference Δ^A	p
Round1	0.64	0.48	0.46	0.50	*-0.03*	*0.39*
Round2	0.39	0.49	0.47	0.50	*-0.01*	*0.69*
Round3	0.47	0.50	0.42	0.50	*-0.07**	*0.08*
Round4	0.44	0.50	0.48	0.50	*-0.00*	*0.90*
Period_total	1.95	1.47	1.83	1.58	*-0.12*	*0.12*
Round_average	0.49	0.37	0.46	0.39	*-0.03*	*0.12*

Scarcity Groups (n=115)	**Period 1** mean	sd	**Period 2** mean	sd	Within-subject difference Δ^A	p
Round1	0.55	0.50	0.30	0.46	*-0.17****	*0.00*
Round2	0.46	0.50	0.39	0.49	*-0.08**	*0.06*
Round3	0.38	0.49	0.26	0.44	*-0.21****	*0.00*
Round4	0.49	0.50	0.34	0.48	*-0.13****	*0.00*
Period_total	1.88	1.37	1.29	1.41	*-0.59****	*0.00*
Round_average	0.47	0.34	0.32	0.35	*-0.15****	*0.00*

Between-subject difference	$\Delta P1^B$	p	$\Delta P2^B$	p
Δ^B Round1	*-0.10*	*0.15*	*-0.16***	*0.01*
Δ^B Round2	*0.07*	*0.30*	*-0.08*	*0.21*
Δ^B Round3	*-0.09*	*0.17*	*-0.16***	*0.01*
Δ^B Round4	*0.05*	*0.45*	*-0.14***	*0.03*
Δ Period_total	*-0.07*	*0.78*	*-0.54****	*0.01*
Δ Round_Average	*-0.02*	*0.78*	*-0.14****	*0.01*

A: Within-subject comparisons use period 1 averages as reference

B: Between subject comparisons use the same round from the other treatment as reference

Note: Wilcoxon Signed-Rank tests for p-values in within-subject comparison
Mann-Whitney U-tests for p-values in between-subject comparison
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculations based on collected data

5.1 Within-subject analysis

Being confronted with resource scarcity in the second period, players reduced their clearing significantly in all rounds of the scarcity condition. The total reduction from period 1 to period 2 is 0.59 parcels per player, which equals 0.15 per round and is highly significant at $p < 0.01$ (table 3). The within-subject model (C1) of the regression table confirms through corresponding coefficients that players in the scarcity treatment significantly reduced their clearing in all four rounds from period 1 to period 2 (table 4). In the control group, that played the abundance environment in both periods, there is also a slight reduction in clearing from period 1 to period 2 of 0.12 parcels, which equals 0.03 parcels per round (table 3, table 4). Even though the difference is not significant (table 3), it means that players who played the abundance treatment only, also tend to clear less in the second period of the game. In other words, they increase their level of cooperation over time even without any external change in game conditions.[38,39] This advocates the use of a difference-in-differences comparison to avoid over-estimation of the scarcity effect based on within-subject comparisons only.

5.2 Between – subject analysis

Secondly, differences in clearing in period 2 are to be tested between treatment groups.[40] It turns out that players, who played the abundance environment, clear on average 0.46 parcels whereas groups in the scarcity environment clear 0.32 parcels per round. The difference is highly significant at $p < 0.01$ (table 3). Comparing single rounds, differences are significant in all rounds except the second one. The differences in each round are confirmed by the coefficients of the between-subject regression without control variables (C2). Adding control variables to the model (C3) slightly alters coefficients but the overall statements remain the same. Noteworthy, the values and coefficients (table 3 and 4) in the within- and the between-subject comparisons are actually quite similar to each other, which is not really surprising as there were firstly little initial differences between treatment groups and secondly, the control group did not change their behavior much from period 1 to period 2.

[38] There is no self-evident explanation for this, but it can be hypothesized that the reduction in clearing might be attributable to a learning- or restart-effect that occurs when the game is repeated. Such effects have been found and discussed in other experimental studies on cooperation (Andreoni 1988; Croson 1996; Cookson 2000; Croson 2001; Croson et al. 2005; Eckel et al. 2016; Chaudhuri and Paichayontvijit 2017).

[39] Contrary to abovementioned increases in cooperation, there is also quite some evidence for decreasing cooperation in repeated games over time, e.g., Ledyard (1995), but usually without an explicit "restart" of the game.

[40] An equivalent between-subject regression for the first period confirms that there are no significant differences in clearing rates across treatments in the first period. In other words, the two treatment groups can be considered to start off as equal (see Supplement A).

Table 4: Regression models

Dependent variable: Clearing per round	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)	(C7)
	OLS Regressions			Fixed Effects		Random Effects	
	Within Subject	Between Subject	Between Subject	Plain	DID	Plain	DID
time-invariant controls	*no*	*no*	*yes*	*no*	*no*	*yes*	*yes*
Scarcity_P2_Round1 (d)	-0.174***	-0.160**	-0.140**	-0.174***	-0.143**	-0.174***	-0.142***
	(0.00)	(0.01)	(0.03)	(0.00)	(0.01)	(0.00)	(0.01)
Scarcity_P2_Round2 (d)	-0.078*	-0.082	-0.062	-0.078*	-0.065	-0.078*	-0.064
	(0.06)	(0.22)	(0.35)	(0.05)	(0.22)	(0.05)	(0.22)
Scarcity_P2_Round3 (d)	-0.209***	-0.159**	-0.139**	-0.209***	-0.142***	-0.208***	-0.141***
	(0.00)	(0.01)	(0.02)	(0.00)	(0.01)	(0.00)	(0.01)
Scarcity_P2_Round4 (d)	-0.130***	-0.143**	-0.123*	-0.130***	-0.126**	-0.130***	-0.125**
	(0.00)	(0.03)	(0.06)	(0.00)	(0.02)	(0.00)	(0.02)
Abundance_P2_Round1 (d)				-0.031		-0.032	
				(0.39)		(0.38)	
Abundance_P2_Round2 (d)				-0.013		-0.014	
				(0.68)		(0.68)	
Abundance_P2_Round3 (d)				-0.067*		-0.067*	
				(0.08)		(0.07)	
Abundance_P2_Round4 (d)				-0.004		-0.005	
				(0.90)		(0.89)	
P2_Round1 (d)		0.000	0.000		-0.031		-0.032
		(.)	(.)		(0.39)		(0.38)
P2_Round2 (d)		0.018	0.018		-0.013		-0.014
		(0.73)	(0.73)		(0.68)		(0.68)
P2_Round3 (d)		-0.036	-0.036		-0.067*		-0.067*
		(0.44)	(0.44)		(0.08)		(0.07)
P2_Round4 (d)		0.027	0.027		-0.004		-0.005
		(0.57)	(0.57)		(0.90)		(0.89)
Age			0.007***			0.004**	0.004**
			(0.00)			(0.02)	(0.02)
Female (d)			-0.082*			-0.073*	-0.073*
			(0.08)			(0.09)	(0.09)
Schooling_years			-0.003			-0.007	-0.007
			(0.71)			(0.28)	(0.28)
Adults_in_hh			0.008			0.010	0.010
			(0.44)			(0.33)	(0.33)
Village_native (d)			0.064			0.037	0.037
			(0.21)			(0.44)	(0.44)
Bags_farming_yield			-0.001			-0.001	-0.001
			(0.60)			(0.55)	(0.55)
Hectares_cultivated			-0.020			-0.023*	-0.023*
			(0.14)			(0.06)	(0.06)
Hectares_cleared			0.024*			0.024*	0.024*
			(0.10)			(0.06)	(0.06)
Clearing_rules (d)			-0.033			-0.012	-0.012
			(0.51)			(0.78)	(0.78)
Land_rivalry (d)			-0.109**			-0.086*	-0.086*
			(0.05)			(0.09)	(0.09)
River (d)			-0.123**			-0.094**	-0.094**
			(0.02)			(0.05)	(0.05)
_cons	0.470***	0.455***	0.307**	0.478***	0.478***	0.447***	0.447***
	(0.00)	(0.00)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
N	920.000	908.000	908.000	1816.000	1816.000	1816.000	1816.000
R2	0.046	0.026	0.108	0.028	0.028	0.074	0.074
F	7.92	2.52	3.32	4.41	4.41	67.98	67.98
P>F	0.00	0.02	0.00	0.00	0.00	0.00	0.00

p-values in parentheses
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Overall R2 and Wald-Chi2 instead of F for random effects regressions
Source: own calculations based on collected data

5.3. Panel regression analyses

The scarcity-round coefficients of the fixed effects panel regression (C4) are equal to the values in table 3 and the coefficients of the within-subject regression (C1), but the model also includes observations and coefficients for the control group. One round of the control group actually shows a slightly significant reduction in clearing compared to the first period (table 3, table 4). Overall, the period average of the control group was found to be slightly lower in the second period (table 3). Hence, the DID model is applied in order to verify that the reductions in

clearing rates under scarcity are indeed significantly lower than in the control group. Adding time-fixed effects for round 1 to 4 of the second period interprets the reductions found in the control group as the general time-trend (C5). Consequently, the coefficients for the time-trend effects are the same as for the control group in the fixed effects regression (C4), but the values of the scarcity coefficients diminish by the size of the time trend in the DID-regression (C5). It turns out that reductions in clearing attributed to the scarcity conditions remain significant in all but the second round. The results for the random effects regression models (C6 and C7) show very similar coefficients as both fixed effect models, but also take into account time-invariant variables.

5.4. Effects of exogenous vs. endogenous scarcity

In order to also find out about effects of endogenous resource scarcity, i.e., scarcity that is caused or aggravated by the users' behavior, a regression model is calculated that includes explanatory variables for the players' and their group members' previous clearing behavior. One regression calculation is presented for each period and treatment, using 4 rounds in each block per model (table 5). Consequently, scarcity-treatment dummies can no longer be included as explanatory variables. It turns out that in all three abundance conditions[41] players tend to clear less if more forest has been taken by other group members, but not so in the scarcity condition. Coefficients for this finding are remarkably consistent across all three abundance conditions. It can be concluded that forest stock reductions through a group's own clearing behavior appear to have the same effect as exogenously reduced stocks, i.e., lead to a more conserving usage. This only applies to the abundance conditions though, where appropriation is never competitive. In the scarcity conditions, on the other hand, quickly diminishing resource stocks do not cause a slower extraction.

Further, it is found that one's own clearing behavior in previous rounds is strongly positively correlated with the decision to clear (table 5). The coefficients are larger in the second period for both treatments, indicating that players likely have decided for and adhere to a particular strategy by then.

[41] That is both treatments in period 1 and the control treatment in period 2.

Table 5. Regressions per period and treatment

Dependent variable: clearing per round	(S1)	(S2)	(S3)	(S4)
	All Models Random Effects			
	Abundance Period 1	Scarcity Period 1	Abundance Period 2	Scarcity Period 2
Clearedbefore_self	0.236***	0.222***	0.473***	0.378***
	(0.00)	(0.00)	(0.00)	(0.00)
Clearedbefore_others	-0.013**	-0.012**	-0.012**	0.002
	(0.02)	(0.05)	(0.04)	(0.74)
Age	0.003	0.000	0.003	0.007***
	(0.14)	(0.99)	(0.12)	(0.00)
Female (d)	-0.025	-0.079	-0.060	-0.075
	(0.66)	(0.14)	(0.22)	(0.11)
Schooling_years	-0.008	-0.014	-0.010	0.012*
	(0.30)	(0.10)	(0.11)	(0.09)
Adults_in_hh	0.003	0.021	0.005	-0.002
	(0.82)	(0.12)	(0.57)	(0.83)
Village_native (d)	-0.010	0.004	0.016	0.097*
	(0.87)	(0.95)	(0.75)	(0.05)
Bags_farming_yield	0.001	-0.002	-0.000	-0.000
	(0.56)	(0.14)	(0.94)	(0.70)
Hectares_cultivated	-0.029*	-0.007	-0.019	-0.007
	(0.08)	(0.66)	(0.18)	(0.58)
Hectares_cleared	0.052**	-0.002	0.030*	0.005
	(0.01)	(0.89)	(0.08)	(0.68)
Clearing_rules (d)	0.007	0.051	-0.056	0.012
	(0.91)	(0.37)	(0.30)	(0.80)
Land_rivalry (d)	-0.078	0.029	-0.068	-0.053
	(0.27)	(0.63)	(0.25)	(0.29)
River (d)	-0.031	-0.104	-0.074	-0.163***
	(0.62)	(0.13)	(0.15)	(0.00)
_cons	0.386**	0.571***	0.371***	-0.039
	(0.01)	(0.00)	(0.01)	(0.75)
N	448.000	460.000	448.000	460.000
R2	0.134	0.096	0.227	0.202
Wald-Chi2	77.62	59.24	216.33	149.50
P>F	0.00	0.00	0.00	0.00

p-values in parentheses
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Overall R2 and Wald-Chi2 instead of F for Random effects regressions
Note that the scarcity groups in period 1 played the baseline game, i.e., abundance
Source: own calculations based on collected data

5.5. Real-world scarcity and other variables

The between-subject and random effects regression models (table 4) additionally allow analyzing correlations with socioeconomic variables, which are time-invariant in our panel model. Results show that the age of the participant is positively associated with higher clearing rates, and female participants seem to use the forest resource more carefully than their male counterparts. There is further a positive correlation between clearing in the game and the clearing of forest in the real world as stated by our participants in the post-experimental survey, which supports external validity of the experiment. The total size of fields cultivated by an individual is, however, negatively associated with experimental clearing, which could potentially be explained by the circumstance that those who already own large areas of land do not need to clear new fields as often (and can possibly even rotate on their existent fields).

Interestingly, participants who stated to have experienced rivalry in land acquisition in the past tend to extract less in the experiment. The River dummy, which represents whether participants live in a region of real resource scarcity, confirms this by assuming a negative and significant value (table 4). Split tables for each round and treatment (table 5) reveal an important additional

insight: The reduced clearing associated with the river variable primarily stems from the experimental scarcity treatment, whereas living at the river does not have an effect on experimental clearing in the abundance conditions. The implication is that those who are used to handling resource scarcity in real life and have experienced competition for resources are more likely to conserve forest in the respective experimental game setting, i.e., they are more cooperative, but only under scarcity and not in general. Other control variables, such as economic status indicators, being born in the respective village or the level of education do not show a consistent effect on experimental clearing behavior. Existing real-world rules on forest clearing within the village do not seem to affect experimental clearing decisions, either.

6. Discussion

To sum up, results from all presented perspectives paint a rather clear picture that shows extraction being significantly lower in the scarcity environment compared to the abundance environment. As a most self-evident interpretation, we attribute this difference in behavior to the users' concern for the survival of the resource. The results surprisingly contrast recent field experimental findings from Gatiso et al. (2015) as well as Blanco et al. (2015) and are more in line with the older classroom experiments by Rutte et al. (1987), Osés-Eraso and Viladrich-Grau (2007) and Osés-Eraso et al. (2008). Cooperation most likely arises in situations where it is perceived as needed, in this case for ensuring the survival of the resource (Ostrom 1999; Gibson et al. 2007). This hypothesis finds further support in Kramer (1989), who suggests that a group facing a common, externally induced threat or challenge, such as resource scarcity, might be more inclined to cooperate due to increased group identification and social cohesion.

From a real-world point of view, which goes beyond the scope and mechanisms of the conducted experiment, reduced clearing in scarce environments could also be explained by non-linearity of forest benefits: A very large forest might still yield sufficient benefits even if parts of it are cleared. If there is less forest left, however, further clearing has a more substantial negative impact on the benefits that the remaining forest can generate. In such a situation there would be diminishing marginal social benefits from the forest. In our study private benefits from clearing could possibly diminish when the forest was becoming very small, namely less than 7 parcels, but the social benefit from remaining forest was held constant at 1N$ per parcel. Hence, from a rational point of view, cooperation is at least as reasonable in the abundance condition as it is in the scarcity condition of our experiment. A possible reconciling explanation

could be found in presuming that participants intuitively bring considerations from the real world into the game, even though they do not apply there.

Reduced extraction of resources was observed under both exogenous scarcity as given in the experimental treatment, and under endogenously, through players' clearing behavior, reduced resource stocks. The combination of both, however, i.e., high clearing rates in previous rounds together with an initially scarce stock, is not associated with reduced clearing rates. Such a condition could be understood as extreme scarcity and this observation therefore also points towards a possibly non-linear effect of the resource stock: Once it has become clear that there is no realistic chance for the resource to survive until any social benefits accrue from it, players will give up any conservation effort (cf. Ostrom 1999). In this context it must be noted that higher conservation as the response to more clearing by one's group members is actually somewhat astonishing as the concept of reciprocity and conditional cooperation would predict the opposite (cf. Fischbacher et al. 2001; Croson 2007; Kocher et al. 2008). It could be conjectured that the cooperation-enhancing effect of endogenous scarcity is strong enough to override the effect of reciprocity.

Finally, one particularly interesting finding is that having experienced real-life scarcity makes participants extract less in the experimental scarcity condition. This could mean that those who are used to scarcity have already learned to cooperate in situations where cooperation is muchly needed.

7. Conclusion

It is expected that scarcity of resources will pose increasingly severe problems in the near future; considerations about policy responses must carefully take into account relevant factors of locally given environments. According to our findings, competitive scarcity of resources does not speed up appropriation rates, but more is extracted where stocks are abundant. Potential conservation measures and programs should therefore not neglect resources or areas that appear abundant as the total conservation effect might potentially be greater when targeting abundant stocks or zones first. Measures that restrict access to resources, on the other hand, such as the introduction of protected zones or parks, leaving only a reduced stock to use, must not necessarily lead to higher extraction rates due to increased competition. Local users are able to realize the need for cooperation and self-organization where it arises and act accordingly. This might, however, not apply to non-local users, such as large corporations, which seek to

maximize short-term profits and have the option of moving to other regions after exploiting the resource. Also, different types of resources that show different characteristics in terms of private use and social benefits might provoke different responses to scarcity. Further research in various environments, also non-experimental ones, is therefore advisable.

❖

References Chapter I

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Supplementary Materials Chapter I

A. Equivalents for regression results: Within and between models

B. Game mechanics (extended version)

C. Protocol for workshop

C.1 Village meeting

C.2 General instructions

C.3 Game instructions for forestry and agriculture game

C.4. Examples

C.5: Decision making and debriefing

C.6 Treatment conditions

D. Survey questions

E. Information for data and analysis script request

Supplement A: Equivalents for regression results: Within and between models

Dependent variable: Clearing per round	(EQ1) within b/p	(EQ2) between b/p	(EQ3) between_c b/p
Abundance_P2_Round1 (d)	-0.031		
	(0.39)		
Abundance_P2_Round2 (d)	-0.013		
	(0.69)		
Abundance_P2_Round3 (d)	-0.067*		
	(0.08)		
Abundance_P2_Round4 (d)	-0.004		
	(0.90)		
Scarcity_Eq_R1(d)		-0.095	-0.075
		(0.15)	(0.25)
Scarcity_Eq_R2(d)		0.068	0.088
		(0.30)	(0.19)
Scarcity_Eq_R3(d)		-0.091	-0.070
		(0.17)	(0.29)
Scarcity_Eq_R4(d)		0.049	0.070
		(0.46)	(0.31)
Round_1 (d)		0.000	0.000
		(.)	(.)
Round_2 (d)		-0.250***	-0.250***
		(0.00)	(0.00)
Round_3 (d)		-0.170***	-0.170***
		(0.00)	(0.00)
Round_4 (d)		-0.205***	-0.205***
		(0.00)	(0.00)
Age			0.002
			(0.43)
Female (d)			-0.063
			(0.18)
Schooling_years			-0.011*
			(0.10)
Adults_in_hh			0.013
			(0.30)
Village_native (d)			0.010
			(0.85)
Bags_farming_yield			-0.001
			(0.59)
Hectares_cultivated			-0.026*
			(0.06)
Hectares_cleared			0.024*
			(0.08)
Clearing_rules (d)			0.009
			(0.86)
Land_rivalry (d)			-0.063
			(0.25)
River (d)			-0.065
			(0.22)
_cons	0.487***	0.643***	0.708***
	(0.00)	(0.00)	(0.00)
N	896.000	908.000	908.000
R2	0.004	0.025	0.064
F	0.85	5.14	3.45
P>F	0.49	0.00	0.00

p-values in parentheses
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Scarcity_Eq_RX is a dummy variable for players who will later play the scarcity condition in period 2. This Equivalent variable measures if there is already a difference in the first period, when all players were actually playing the same abundance condition.
Source: own calculations based on collected data

Supplement B: Game mechanics (extended version)

I. The Abundance Environment (baseline and control treatment)

The game is designed as a dynamic common-pool resource game where the social benefit occurs at the end (of the period). Since there are no discount rates used over the rounds, each decision equally affects the final private and social outcome and the personal payoff can, for the baseline (abundance) game, effectively be written as the sum of all 4 rounds. The order of decisions does therefore not make any difference in economic terms. It might however play a behavioral role, as in the dynamic game players will learn about the other group members' decision after each round and possibly respond to that by behaving in a certain way in the following rounds.

Hence, while the social benefit of the forest is framed in a way that it occurs at the end of the game, the payoff of a player (R) can also be written for each round individually: Player *i* makes a binary decision in each round to either clear or not clear a new field. Let c_i be 1 when player *i* decides to clear and 0 when they decide to stay. $\sum c_j$ is the sum of clearing by the other group member in that round. Then,

Equation (1):

$$\boldsymbol{R_i(c_i, c_j) = 3.50 * c_i + 1 * (1 - c_i) + 1 * (1 - c_i) + 1 * (6 - \sum_{j=1}^{6} c_j)}$$

which can be reduced to

$$\boldsymbol{R_i(c_i, c_j) = 8 + 1.50 * c_i - \sum_{j=1}^{6} c_j}$$

R_i = payoff player *i* in any round

c_i = decision by player *i* (0 = stay, 1 = clear)

c_j = decisions by other players *j={1,2,3,4,5,6}* (0 = stay, 1 = clear)

For the whole game over 4 rounds (r) the total payoff of a player (P) can be written as a function of all decisions made by them and the other players. Their total payoff (P) then equals the sum of the payoffs (R) from the single rounds summed up over all four rounds. Note that these considerations do not apply for the scarcity treatment as here the possible decisions in the later rounds depend on all players' previous decisions. The game can therefore not be split up into four equal rounds as it can be in the abundance case.

Therefore:

Equation (2):

$$\boldsymbol{P_i = \sum_{r=1}^{4} R_{i,r}\left(c_{i,r}, c_{j,r}\right) = 3.50 * \sum_{r=1}^{4} c_{i,r} + 1 * \sum_{r=1}^{4}\left(1 - c_{i,r}\right) + 1 * \sum_{r=1}^{4}\left(1 - c_{i,r}\right) + 1 * \sum_{r=1}^{4}\left((6 - \sum_{j=1}^{6} c_{j,r})\right)}$$

total payoff of player *i*	yield from clearing	yield from staying	benefit from spared forest	benefit from forest spared by others

which can be reduced to

$$\boldsymbol{P_i\left(c_{i,r}, c_{j,r}\right) = 32 + 1.50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}}$$

P_i = total payoff of player *i* in the game

$c_{i,r}$ = decision by player *i* (0 = stay, 1 = clear) in round *r*

$c_{j,r}$ = decisions by other players *j={1,2,3,4,5,6}*, (0 = stay, 1 = clear) in round *r*

r = round = *{1, 2, 3, 4}*

The only remaining variable that can now be manipulated by player *i* is $c_{j,r}$. It easily becomes evident that in each round clearing increases one's individual payoff and thereby for the whole game it is individually optimal to clear a new field in every round. For the group as a whole on the other hand, the maximization of total payoffs is reached if no player clears any new field.

This becomes evident by setting $c_{i,r} = c_{j,r,}$ and summing up all players' payoffs as in Equation (3):

$$\boldsymbol{S(c_r) = 7 * [\,32 + 1.50 * \sum_{r=1}^{4} c_r - 6 * \sum_{r=1}^{4} c_r\,]}$$

S = social payoff of the group, i.e., of all players combined

c_r = decision by each player in round *r* (0 = stay, 1 = clear)

r = round = *{1, 2, 3, 4}*

then,

$$\boldsymbol{S(c_r) = 7 * [\,32 - 4.50 * \sum_{r=1}^{4} c_r\,]}$$

$$\boldsymbol{S(c_r) = 224 - 31,5 * \sum_{r=1}^{4} c_r}$$

As clearing in each round linearly reduces the social payoff, the maximum is found at $c_r = 0$, i.e., no clearing. In this case the social benefit is 224 N$ which equals 32N$ per player per period. If all players act entirely selfish and always clear, the social payoff sums up to only 98N$, which is 14N$ per player; less than half of what can be achieved by mutual cooperation.

II. The Scarcity Environment

After the end of round four the game restarted. For the second period of the game, the control (abundance) group repeated the same game as in the first period, i.e., starting with a new forest stock of 28 parcels. The scarcity treatment group, however, restarted with a reduced stock of only 14 parcels of forest (50% of the control and period 1 stock). The payoffs remained the same: 3.50N$ for a freshly cleared field and 1N$ for staying on an old field. At the end of round 4 each player receives 1N$ for every parcel of forest that is left.

With the reduced "scarcity" stock of 14 parcels, it is possible that the forest gets depleted by the end of round 2 in the extreme case that all 7 players always clear. Consequently, in case there is no forest left, no more clearing is possible but all players receive a yield for staying of 1N$ for the remaining rounds of the game. Before playing round 3 and round 4 it can also happen that there are less than 7 parcels of forest left. If in those rounds more players decide to clear than there are parcels left, the yield for clearing is reduced from 3.50N$ to 2N$. We set this as a simple rule in order to keep understanding facile and to avoid calculating with fractional numbers.

Receiving a reduced clearing yield of 2N$, players should be theoretically indifferent between clearing and staying as 1N$ from staying plus 1N$ from social benefits that accrues from the spared parcel of forest in the end sums up to the 2N$ that can alternatively be earned right away by clearing. This consideration does however need to be extended and altered due to two reasons: Firstly, no player knows what the other players in their group are going to decide in the current round. Therefore, there is still the possibility of getting 3,50N$ and thereby increasing one's payoff by clearing in case the numbers of players clearing does not exceed the numbers of forest parcels left. And secondly, even if the expected gain from clearing is only 2N$, it could happen that the rest of the forest is just taken by the other players, leaving no social benefits for anyone in the end. In this case, 2 N$ is still preferable to 1$ from staying and clearing hence remains the individually best option at all times.

Even if a player or a group of players restrain themselves from excessive clearing, the forest stock could easily be depleted by the other users in the scarce environment. While in the abundance treatment each spared parcel of forest leads to a safe social benefit, in the scarcity treatment there is competition for resources and uncertainty about whether conservation efforts actually result in any social benefits in the end. In economic terms, this means the marginal per capita return of cooperating (i.e., of not-extracting) is unknown as it depends on the other players' decisions. It can become zero if the spared resources are instead entirely taken by the other players. Note that no mathematical equation as for the baseline (abundance) game is provided for the scarcity treatment as it would results in too many possible cases once less than seven forest parcels are left. Even so, the scarcity treatment does not change the individually or socially optimal decisions in the game compared to the abundance environment. It remains individually optimal to clear and socially optimal not to clear. This also holds if the forest stock drops below 7 parcels in the later rounds, but then the additional considerations about potentially reduced yields due to scarcity come into play. In order to avoid last-round effects, players were not told in advance how many periods, i.e., repetitions of the game they were going to play in total. There was indeed a third period of the game played after the end of period 2, but observations from this third period are part of another study, and therefore not discussed here.

Supplement C: Experimental workshop protocols:

C.1 Village meeting

Village Meeting

[freely presented by Christian, interpreted by Moses (assistant)]

To begin with, we would like to thank you all for coming here today. My name is Christian Hoenow. I am from the Marburg University in Germany. Together with the Ministry of Agriculture, Water and Forestry we are conducting research under the SASSCAL project. [NAME OF EXPRIMENTERS] here are also part of the project.
Doing research means we are just here to collect data, but we do not bring any type of development project into the village. What you answer in the workshop will not determine whether villages are selected for future projects. Also, we are not here to teach you anything. On the contrary, we want to learn from you. About how you do agriculture and how you use the forest. These information will help us identify potential problems and come up with possible solutions. Such solutions however will not be brought by us. We only write down what we find and then the information can later be used by the Government or by Organizations.
Today we would like to conduct a small workshop with a certain number of people. At the end of the workshop, we will also ask you several questions one by one. Unfortunately, not everyone from this village can participate since the workshop can only include a certain number of participants (28). Since we want everyone to have the same chance to participate, we have prepared a bag with as many cards as people present. Each adult that is older than 18 years may draw a card. We will ask you to fully concentrate on the workshop and we will be asking many questions. If you already know that you cannot attend for up to 5 hours, or do not wish to answer many questions, you should please not draw. Participation is, of course, voluntary!

- If you draw a red card, you will participate in the workshop, which is led by Cypriaan
- If you draw a yellow card, you will participate in the workshop led by Moses
- If you draw a blue card, you will participate in the workshop, which is led by Anastasia
- If you draw a green card, you will participate in the workshop led by Blondy
- If you draw a white card, you unfortunately cannot participate in any of the events.

Do you have any questions?
[LET EVERY ADULT DRAW A CARD]
[CONTINUE WITH GENERAL INSTRUTIONS]

C.2 General instructions

General Instructions

To begin with, thank you again for coming here today. We will conduct a workshop where you will earn real money. Different participants may receive different amounts of money. The money that you can earn is not our private money, but it is provided by the German government.
All information collected today will be used for research only. Neither the Government of Namibia or Germany nor any other organization will receive the data for other purposes. Also, neither your names nor any village-specific information will be linked to the results. All decisions made will remain anonymous to others.
The schedule for today looks as follows:

1. We will explain the procedure of the workshop.
2. We will play small workshop like a game. This is when you can earn money.
3. After the games each of you answers a short survey-questionnaire.

It is not the purpose of the game to be better than others. Also, there are no right or wrong answers and we do not expect anything in particular from you. All payments are determined exactly the way we will explain to you later.
Before starting, I would like to give you some general information:

1. If at any time, you think that this is something that you do not wish to participate in for any reason, you are free to leave. You will however only get all money you earned if you stay until the end of the workshop.
2. If you already know that you will not be able to stay for at least 5 hours, then you should leave right away.
3. We require your complete and undistracted attention. Please, follow the instructions carefully and do not use your phone or engage in any other distracting activity.
4. It is not allowed to talk to each other during the workshop, unless we tell you to. You can ask questions after raising your hand. If you talk to each other when you are not allowed to, you will be excluded from the workshop and the payments.
5. Every one of you has received a unique player ID. Please keep this ID until the end. You must return the ID before receiving the money at the end of the workshop.

After knowing these rules, is there anybody who does not wish to participate anymore?
Do you have any questions?

C.3 Game instructions for forestry and agriculture game

[Seat groups 1-7 in blocks according to their ID color and number]
I will now explain what you will do in the workshop today. Please pay attention to what I say, as it is important that you understand everything. We have allocated you into three groups of 7 people each. I am the instructor and will tell you what the rules are, answer questions if there are any and let you know about the outcomes. We will play something similar to a game. I do not participate in the game myself. The game is played within your group of 7 people only.

In this game you will earn real money. The amount you receive depends on your decisions and the decision of other players. The numbers we mention in the game are exactly real dollar. We will note for each of you how much you earn during the game and in the end, we will give you the money in Namibian Dollars. In total you will earn between 24 and 120 Namibian Dollars at the end of the day.

It is not the purpose of the game to be better than others. There is no right or wrong, good or bad. It is possible that some players get more than other, that all players get a lot or that all players get only little. It all depends on your decisions in the game and the decisions of your group members.

The game we play is about agriculture and forest use. You will be making decisions about whether you want to clear forest in order to clear a new field or if you want to continue using your old field.
There is a forest of 28 hectares [show at board, TABLE_FOREST, 7 x 4 trees] that belongs to your group. You will play the game over 4 rounds. In the beginning the size of the forest is 28 hectares.
As you know, the soil fertility decreases over time. In the game we have old and new fields. Each player starts with an old field that will give you 1$ per round. If you decide to clear forest to get a new field, you will earn 3,50$ in that round. In every round you can decide to clear a new field. That means you clear one hectare of forest and cultivate a new field there. Thus, each clearing causes the forest stock to decrease by one hectare. A new field will give you 3,50$ only once. In the next round, the new field becomes an old field and gives you 1$, unless you clear a new field again. Clearing means you leave the old field and again get 3,50$ instead of 1$.
We are aware that in reality some of you are cultivating more than one hectare and more than one field at the same time because they have a larger household and better equipment. But for this game, every player can only cultivate one field at the same time. That means, when you decide to clear a new field, you will only be using the new field and leave the old one fallow. Also, we know that the soil fertility is slowly decreasing over time, so that in reality there are more conditions than just new and old fields.

However, we want to keep the game simple and are therefore only playing with old and new fields.
For the game this works as follows: In the first round you have to decide whether you want to clear a new field or stick to your old one. If you stay in the old one you will get 1$ in the first round. If you clear and cultivate a new field you will get 3,50$ for the first round.

Every round you make the decision to start with a new field or stay on the old one.
[**Show example for decisions:**
e.g., "Clearing twice" : 3,50 + 1 + 3,50 + 1 (1st and 3rd round)
or "Clearing three times" : 3,50 + 3,50 + 1 + 3,50 (1st, 2nd, and 4th round)]

It is possible to clear a new field in every round, which means you get 3,50$ in every round. As you see, each of you will make 4 decisions in total. Remember that there is 28hectares of forest and there are 7 players in the group. So even if everyone is clearing every round there will be 7player = 7 hectares that they can clear per round. Times 4 rounds equals 28 hectares. It is therefore not possible to deplete to whole forest before the end of the game, even if everyone is clearing new forest all the time. [FIGURE_FOREST]

So, this is the agricultural side of the game and obviously it would be reasonable to start a new field every round in order to get the highest yields from the fields. The fields are your own and therefore the yields you receive from your field are your own personal money that you can keep. There is however also another part of the game which is about the forest. You are aware that forests are not worthless but are valuable natural resources for everyone. This could for example be fruits, mushrooms, timber or firewood that you collect from the forest. Also, cattle can graze in the forest, especially during times of harvest. Finally, forests play an important role in keeping the environment healthy and sustainable. And a large remaining forest can be used by future generations for both forest benefits and leaving options for new fields. These benefits are represented in the game by a payoff that derives from the remaining forest at the end of the game. In particular, everyone will receive 1$ for every hectare of forest that is left after the end of the last round.

The number of hectares of forest left in the end depends on how much was cleared in previous rounds by you and your group members.
It is important to know that the forest benefits go to everyone. Whereas fields are owned and cultivated by one player only, the benefits from the forest at the end go to everyone equally, as nobody owns parts of the forest.
By each time you clear, you decrease the forest by one hectare. Each time you do NOT clear, it means that there will be a hectare of forest remaining which will give you and every other player 1$ in the end.

We will now do a test round for the decision making. This is not the real decision that will affect your payoffs, but it is just meant to help you understand how it works.

We will not make the decisions publicly but we will be using the tablet computers.

Therefore, all the decision you will make remain completely anonymous. The other players in your group will not find out how you decided. They will only see after each round, how much forest was cleared by the group as a whole.

[Show FIGUR_FOREST & FIGURE_DECISION]

[show TABLE_DECISION].

- Remember to not show to your neighbors what you decide
- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.
- Only push the tablet **lightly** and **shortly**. We can also use this **pen**.
- The selected answer will be marked **orange**. You can still **change** your decision.
- When done, swipe **right** and **hand the tablet back** to me/assistant.

[Let Players try test round on tablet]
Let's now look at some examples (write down numbers of examples on the whiteboard! TABLE_EXAMPLES):

C.4 Examples

EXAMPLE 1:
I will use this table to make examples of how players can decide. If a player decides to stay, he/she gets 1$ **[mark].** If a player decides to clear he/she gets 3,50$ **[mark]**

[erase again]

Let's go through an example:

Imagine every player was just sticking to their old field over all rounds. Everyone then gets 1$ in each round, so 1+1+1+1 = 4$ over the 4 rounds.

In addition to that yield, there is the benefit that comes from the forest. Since nothing was cleared, the forest size remains at its initial stock of 28 hectares. For every hectare, each player receives 1 Dollars. That is 28x1 = 28 Dollars.

Remember, that the forest benefits are the same for everyone as everyone is using the same forest.

Your field on the other hand is your own and only generates benefits for yourself.
"As no one cleared, the forest size remains large and there is a lot of forest benefits for the group."

Summing up the benefit from the forest and the yield from the field, everyone receives 4 + 28 = 32 $.

Round 1	1	1	"				
Round 2	1	1	"				
Round 3	1	1	"				
Round 4	1	1	"				
SUM Yields	4	4	4	4	4	4	4

Remaining Forest = 28

SUM Total	32	32	32	32	32	32	32

EXAMPLE 2:
Let's go through another example together: Now imagine one out of the seven players decides to clear a new field in all 4 rounds. He/she will then get 3,50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$. Then the other 6 players who stay on their old field all the time still only get 1+1+1+1 = 4$ from their fields.

Since he/she cleared in every round and no one else did, the forest decreased by one hectare every round. Which is 4 hectares in total. The remaining forest in the end of the game is then 28 – 4 = 24 hectares. Remember that everyone gets 1$ per hectare of the remaining forest at the end of the game, which is 24$ for everyone.

The 6 players who did not clear do then get their agricultural yield of 4 + the forest benefit of 24 = 28$ in total.

"The one player who decided to clear every round receives 14 from agricultural yields + the same 24 from the remaining forest = 38$ in total. Which is more than what those who did not clear got.

Those other 6 player who did not clear however, just as in the previous example, now get 28 in total which is because the forest size decreased to 24 hectares from the clearing of player one"

Round 1	3.50	1	"				
Round 2	3.50	1	"				
Round 3	3.50	1	"				
Round 4	3.50	1	"				
SUM Yields	14	4	4	4	4	4	4

Remaining Forest = 24

SUM Total	38	28	28	28	28	28	28

EXAMPLE 3:
But now imagine all 7 players decide to always clear a new field in all rounds. They will then get 3,50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$ for everyone.

Since everyone cleared in every round, the forest decreased by 7 hectares every round. Which is 7*4 =28 hectares in total. There is therefore no forest remaining at the end of the game.

All players do then get their agricultural yield of 14 + but nothing from the forest, so 14$ in total.

"Note that when ALL PLAYERS decide to always clear, each players' final payoff is much smaller compared to when most of the forest stock is conserved.

If the whole group does not clear, then the group as a whole get the most. If you personally decide to clear, you always get more, but the other players in the group will get less. So, when everybody decides to clear a lot, the group benefits from the forest become smaller or vanish and in total everybody is getting less."

Round 1	3.50	3.50	"				
Round 2	3.50	3.50	"				
Round 3	3.50	3.50	"				
Round 4	3.50	3.50	"				
SUM Yields	14	14	14	14	14	14	14

Remaining Forest = 0

SUM Total	14	14	14	14	14	14	14

EXAMPLE 4:
Let's go through another example together: Now imagine one out of the seven players decides to always clear a new field. He/she will then get 3.50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$. 4 Players decide to clear twice, for example in round1 and in round 3. They then get 3,50+1+3,50+1 = 9$ from their fields. The remaining 2 players only clear once and therefore get 3,50+1+1+1 = 6,50$ from their fields.
Now the forest has decreased by 4 hectares because of the clearing by the first player. By 4*2 = 8 hectares from clearing by the four players. The last two players who only cleared once caused a decrease of 2*1 = 2 hectares over all round. In total the forest size therefore decreased by 4 + 8 + 2 = 14hecatres. 28 – 14 = 14 hectares of forest that are left in the end. Remember that everyone gets 1$ per hectare of the remaining forest at the end of the game, which is 14$ for everyone.
The one player who decided to clear every round receives 16 from agricultural yields + 14 from the remaining forest = 28$ in total.

The four players who cleared twice each do then get their agricultural yield of 9 + the forest benefit of 14 = 23$ in total.

The two players who cleared once each do then get their agricultural yield of 6,50 + the forest benefit of 14 = 20,50$ in total.

"We can see that those, who clear a new field more often, get higher payoffs in the end. If however everyone decides to clear all the time, the forest benefits will vanish and everyone's final payoffs decrease" **[compare Ex.3].**

Round 1	3.50	3.50	3.50	3.50	3.50	3.50	3.50
Round 2	3.50	1	1	1	1	1	1
Round 3	3.50	3.50	3.50	3.50	3.50	1	1
Round 4	3.50	1	1	1	1	1	1
SUM Yields	14	9	9	9	9	6.50	6.50

Remaining Forest = 14

SUM Total	28	23	23	23	23	20.50	20.50

Should we have another example?

C.5 Decision making and debriefing

[spatially separate groups now according to their color, set them in order 1-7]
(e.g., at different sides of a building, behind the car, under different trees etc.)

Then, before we start with the game, we would like to ask you some questions. This is just to test whether you all understood the rules of the game. Remember that during the game you are not allowed to talk to each other. If there is anything unclear please ask us now and we will explain it to you. If there is anything unclear later during the game, you can still ask questions, but you will have to raise your hand and I will come to you to help you in private. So, are there any questions right now?

[**ask and answer if so, give another example if requested]**

1. **The more hectares of forest the group has cleared, the smaller becomes the forest stock. True or False?**
   [True]

2. **The benefit you receive from the forest is the same for everyone. True or False?**
   [True]

3. **The more often I clear and start a new field, the more money I will get in the end. True or False?**
   [Always True, does not depend on what the other players do]

4. **The more often the other players clear, the less money I get in the end. True or False?**
   [True, others clearing means decreased forest and decreased forest benefits for me]

The outcome does not depend on chance or luck but only on you and your group members' decisions. We promise that we do all the calculation correctly as explained in the game instructions above and will not deceive you at any stage of the game. Also, there is absolutely no possibility of you or any other player cheating or not playing according to the rules given, as you can only decide in every round if you want to clear a new field or not. We will collect your answers using these tablet computers. They are very easy to use and we will show you how it works. You only have to press "clear" or "not clear = stay" in the screen. Here we have a picture of how it looks like [Show TABLE_DECIDE]. If anything is unclear please ask us for help and do not ask the other players. We will calculate and announce to everyone how much of the forest is left after each round. [Show TABLE_FOREST] (Our calculations are done by the computer and must therefore not be questioned). You will however not learn who cleared or who did not. You only see how much forest is remaining after each round. All the decisions you make will be kept anonymous and no one will find out what the other players did. Even after the end of the game, you are not obliged to tell anyone what you and the other players decided.

Procedure: You and your group members will make your decision in each round. Here [point TABLE_DECISION] you have to press whether you wish to stay on your old field or start a new one in every round. You are not allowed to talk to each other while the others make their decisions and while we calculate the results. After each round we will tell you how many forest parcels are left. After the last round (which is after four rounds) the game ends. You will again be informed about the final forest size, which equals the benefits that everyone receives from the forest. It is however not the end of today's workshop. We will afterwards continue with another game.

Remember, that you must not talk to each other and your decisions will remain anonymous.

ARE THERE ANY QUESTIONS?

Then let us now start with the game decisions. We start with the first round out of four and each of you can please make his/her decision on the tablet. [show TABLE_DECISION].

- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.
- Then swipe **right** and **hand the tablet back** to me.
- Remember to not show to your neighbors what you decide

[make decisions]

Thank you. This was the first round. [YY] hectares of forest were cleared. The remaining forest is [XX] hectares [mark on TABLE_FOREST]

We continue with the 2nd / 3rd / last (4th) round out of four and each of you please make his/her decision on the tablet. [show TABLE_DECISION].

- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.

[make decisions]
[YY] hectares of forest were cleared. The remaining forest is [XX] hectares [mark on TABLE_FOREST]

Thank you for playing. The final remaining forest size is XX hectares. That means everyone will receive XX dollar from the remaining forest in addition to their agricultural yields.

Has anything been unclear during the first game? If so please ask us now. We will now continue with another game that works very similarly.

[REPEAT GAME 3x]:

[Period 1: Baseline]

[Period 2: ---See treatment conditions!---]

[Period 3:] We will now play the same game again. That means we start again with a forest stock of 28 hectares. The decisions, payoffs and results from the first game do in no way influence this second game. However, the rule we introduced in the previous session is abolished again [only if treated, nothing abolished for control group]. *(Note: This period not relevant for Scarcity Experiment)*

Debriefing and Control Questions

[AFTER ALL HAVE MADE THEIR DECISION]

Then, before we have a break, we also ask you to answer **two short questions** for understanding individually. This is just meant to help us as feedback to check if everyone understood the game.

[go to experimenter individually, try to keep groups separated so that previous players cannot go back to talk to others**]**

1. Imagine you clear a new field once and everyone else in the group clears a new field three times. Who will get a higher payoff in the end? You, the others, or same for everyone?

2. Imagine you clear a new field two times and everyone else in the group never clears a new field. Who will get a higher payoff in the end? You, the others, or same for everyone?

This is now the end of the first part of the workshop. You are now allowed to talk to each other again.

We will have some snacks and drinks and afterwards continue with a short survey-questionnaire that each of you please answer one after another. When that is done, we will do the payments and we are done.

C.6 Treatment conditions

[Treatment Control Group]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.

[Treatment Scarcity]

Thank you for your decisions. We will now play the game again. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one.
Everyone also still receives 1$ for each tree that remains in the end.
There is however an addition to the game that we will have:

There is now a forest of ONLY 14 hectares [show at board, TABLE_SARCITY, 7 x 2 trees] that belongs to your group. You will play the game over 4 rounds. In the beginning the size of the forest is 14 hectares.
As there are only 14 hectares of forest, it might happen that after some rounds no forest is left.
If all players decide to clear for the first two rounds for example, then the forest decreases by 7 hectares in each round. This is 2x7 = 14 hectares in total. This means there no forest left after the second round.
When there is no forest left, then no one can clear a new field anymore.

Once the forest stock drops below 7, i.e., 6 hectares or less, then the yields will be divided. For example, when there are only 3 hectares left, but 6 players decide to clear. Then each of the 6 players will get 2$ instead of 3.50$ from the new field. This is because they can only each clear and cultivate half a hectare of new field. To keep it simple, we say that whenever the number of players who want to clear in a round is larger than the remaining hectares of forest, those who wish to clear only get 2$ from clearing instead if 3.50$.

The game does NOT end when the forest is depleted, but it always goes over 4 rounds. When no forest is left, however, then no one can clear new fields anymore but all players have to stick to their old field and get 1$ for the remaining rounds.

If there is enough forest left by the other players, then it is possible for you to clear a new field in every round, which means you get 3.50$ in every round.

[show this in FIGURE_SCARCITY]

EXAMPLE Scarcity 1:
Imagine all 7 players decide to always clear a new field. They will then get 3,50$ in the first two rounds.

Since everyone cleared in every round, the forest decreased by 7 hectares every round. Therefore, after the 2nd round no forest is left. As the forest is depleted, all players only get 1$ for the remaining 2 rounds. Over 4 rounds that is 3,50+3,50+1+1 = 9$ for everyone
All players do then get their agricultural yield of 9 but nothing from the forest, so 9$ in total.

Round 1	3.50	3.50	3.50	3.50	3.50	3.50	3.50
Round 2	3.50	3.50	3.50	3.50	3.50	3.50	3.50
Round 3	1	1	1	1	1	1	1
Round 4	1	1	1	1	1	1	1
SUM Yields	9	9	9	9	9	9	9

Remaining Forest = 0

SUM Total	9	9	9	9	9	9	9

EXAMPLE Scarcity 2:
Imagine every player was clearing in the 1st round. Everyone then gets 3.50$ in the first round from their yield. There are then 14-7 = 7 hectares left after the first round.
In the 2nd round 5 players decide to clear. There are then 7-5 = 2 hectares of forest left after the second round.

In the 3rd round 4Players decide to clear. As there are only 2 hectares left, each player gets = 2$ from clearing. Then, after the 3rd round, there is not forest left.
For the 4th round there is no decision to make, as there is no forest left to clear. Every player just has to stay on their old field, getting 1$ from it.

Round 1	3.50	3.50	3.50	3.50	3.50	3.50	3.50
Round 2	3.50	3.50	3.50	3.50	3.50	1	1
Round 3	**2**	**2**	**2**	**2**	1	1	1
Round 4	1	1	1	1	1	1	1
SUM Yields	10	10	10	10	9	6.50	6.50

Remaining Forest = 0

SUM Total	10	10	10	0	9	6.50	6.50

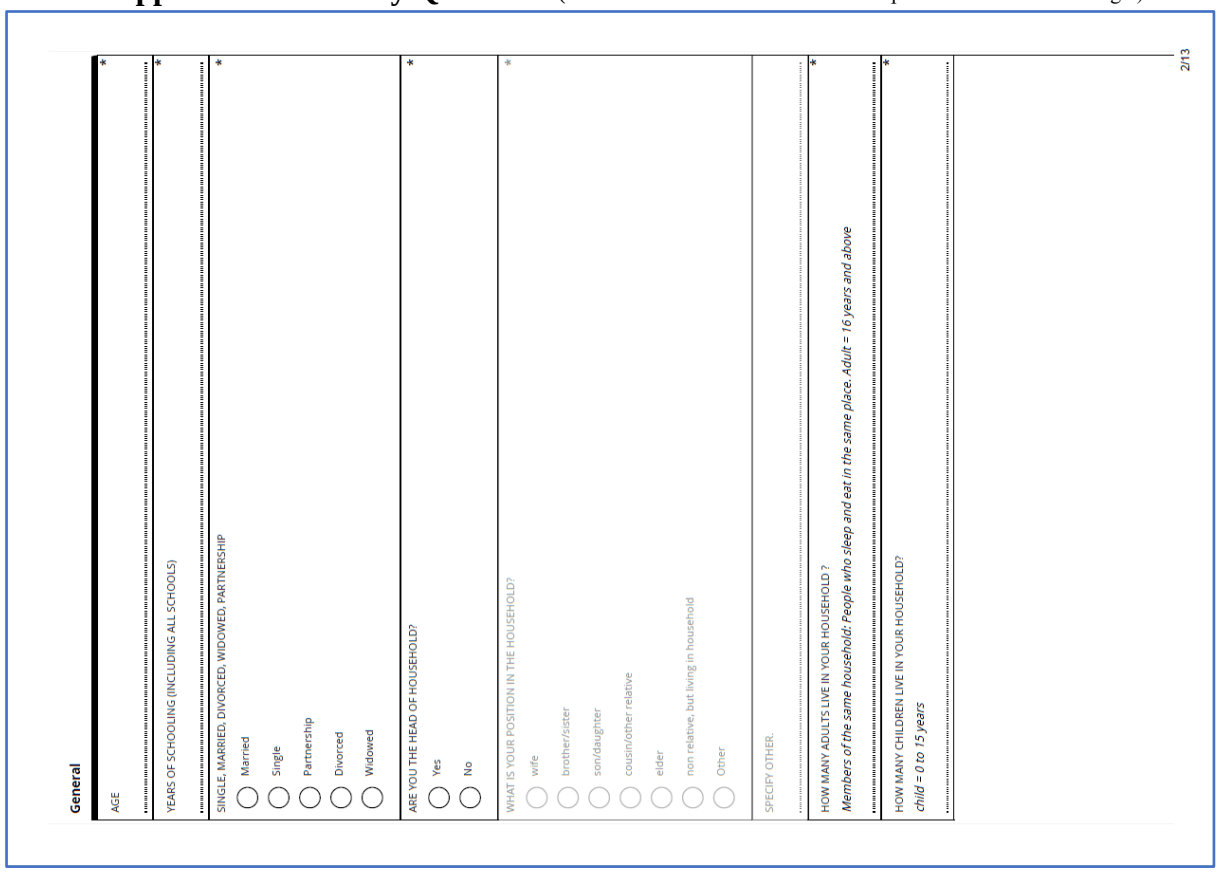
EXAMPLE Scarcity 3:
If everybody however just stays on their old field, then everybody will earn 18$

Round 1	1	1	1	1	1	1	1
Round 2	1	1	1	1	1	1	1
Round 3	1	1	1	1	1	1	1
Round 4	1	1	1	1	1	1	1
SUM Yields	4	4	4	4	4	4	4

Remaining Forest = 14

SUM Total	18	18	18	18	18	18	18

Supplement D. Survey Questions (Sheets made with Kobo-Toolbox: "https://www.kobotoolbox.org/")



General

AGE *

YEARS OF SCHOOLING (INCLUDING ALL SCHOOLS) *

SINGLE, MARRIED, DIVORCED, WIDOWED, PARTNERSHIP *
- Married
- Single
- Partnership
- Divorced
- Widowed

ARE YOU THE HEAD OF HOUSEHOLD? *
- Yes
- No

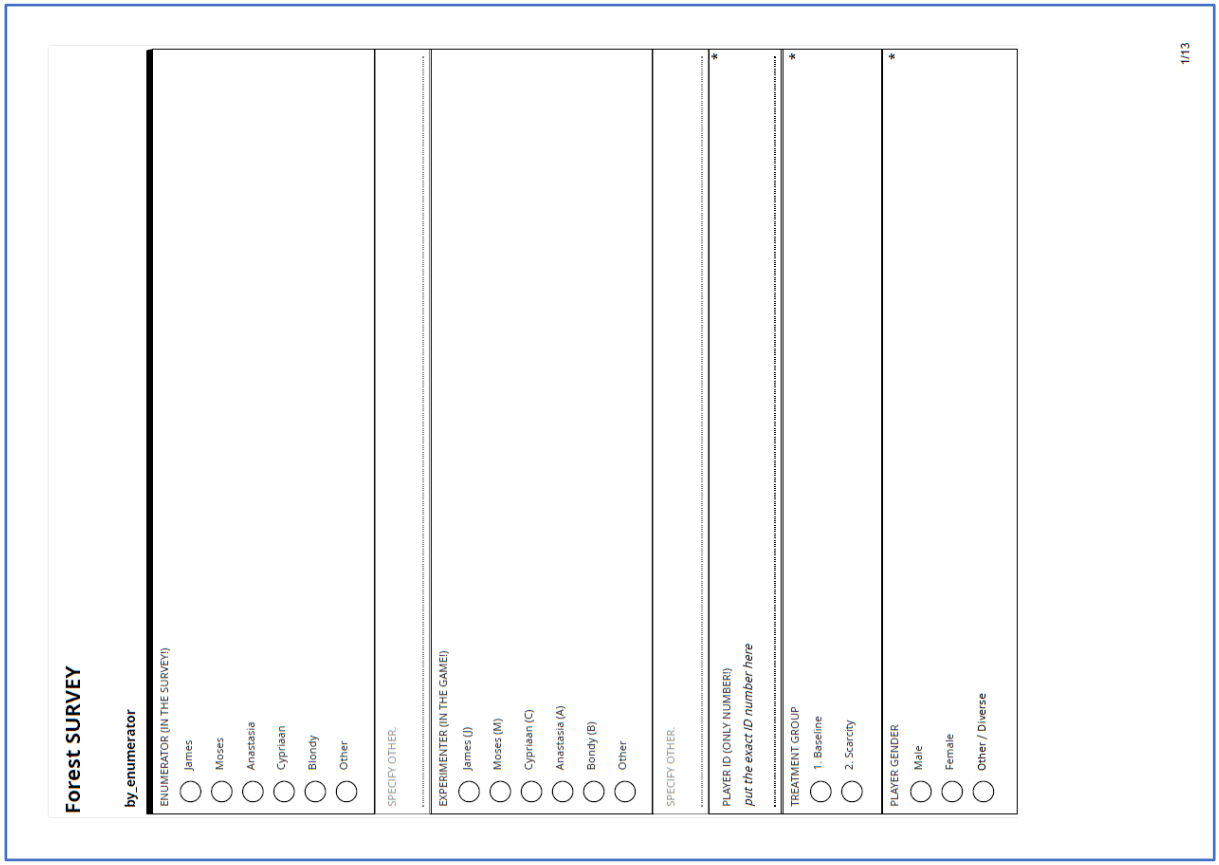
WHAT IS YOUR POSITION IN THE HOUSEHOLD? *
- wife
- brother/sister
- son/daughter
- cousin/other relative
- elder
- non relative, but living in household
- Other

SPECIFY OTHER.

HOW MANY ADULTS LIVE IN YOUR HOUSEHOLD ? *
Members of the same household: People who sleep and eat in the same place. Adult = 16 years and above

HOW MANY CHILDREN LIVE IN YOUR HOUSEHOLD? *
child = 0 to 15 years

2/13



Forest SURVEY

by_enumerator

ENUMERATOR (IN THE SURVEY!) *
- James
- Moses
- Anastasia
- Cypriaan
- Blondy
- Other

SPECIFY OTHER.

EXPERIMENTER (IN THE GAME!)
- James (J)
- Moses (M)
- Cypriaan (C)
- Anastasia (A)
- Bondy (B)
- Other

SPECIFY OTHER.

PLAYER ID (ONLY NUMBER!) *
put the exact ID number here

TREATMENT GROUP *
- 1. Baseline
- 2. Scarcity

PLAYER GENDER *
- Male
- Female
- Other / Diverse

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WHAT IS YOUR RELIGION? *

- ◯ Lutheran
- ◯ Catholic
- ◯ Traditional
- ◯ Evangelic
- ◯ None
- ◯ Don't know
- ◯ Refuse to answer
- ◯ Other

SPECIFY OTHER.

YOUR MOTHER TONGUE? *

- ◯ English
- ◯ Portugues
- ◯ RuShambyu
- ◯ RuKwangali
- ◯ RuNyemba
- ◯ Gciriku
- ◯ Mbukushu
- ◯ Kimbundu
- ◯ Chokwe
- ◯ San
- ◯ Oshiwambo
- ◯ Nama/Damara
- ◯ Afrikaans
- ◯ Otjiherero

3/13

Group_Relations

HOW MANY OF THE OTHER 6 PLAYERS IN YOUR GROUP ARE:

RELATIVES OR SOMEONE FROM THE SAME HOUSEHOLD *

FRIENDS (BUT NOT RELATIVES) *

ACQUAINTANCES *
(Mugeni)

UNKNOWN *

Methodical_Questions

DO YOU THINK THE GAME SOMEHOW REPRESENTS A REAL PROBLEM HERE IN YOUR VILLAGE? *

- ◯ Yes
- ◯ No

WHY DID IT NOT REPRESENT A PROBLEM IN YOUR VILLAGE? *

DO YOU THINK YOU DECIDE IN REAL SITUATIONS SIMILARLY AS IN THE GAME? *

- ◯ Yes
- ◯ No

BEFORE THE GAME STARTED, DID YOU EXPECT THE OTHER PLAYERS TO CLEAR ALL THE TIME, SOMETIMES, RARELY OR NEVER? *

- ◯ all
- ◯ a lot
- ◯ some
- ◯ little
- ◯ nothing
- ◯ Refuse to answer

DID THE FEEDBACK ABOUT THE REMAINING FOREST AFFECT YOUR DECISIONS IN THE FOLLOWING ROUNDS? *

- ◯ Yes
- ◯ No

DO YOU THINK THAT THE OTHER PLAYERS IN YOUR GROUP WERE EXPECTING A CERTAIN BEHAVIOR FROM YOU? *

- ◯ Yes
- ◯ No

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DO YOU THINK THE PAYMENTS OFFERED ARE LITTLE, FAIR OR TOO MUCH? (REAL PAYMENTS NOT MADE YET, BUT ROUGHLY KNOW THE MIN AND MAX AMOUNT TO EARN) *

- ◯ too much
- ◯ fair
- ◯ too little

WHAT DO YOU THINK IS THE REASON FOR CONDUCTING THESE GAMES?
DO NOT READ ANSWERS

- ☐ Qualify for village support
- ☐ find out about the community
- ☐ distribute money
- ☐ don't know
- ☐ Other

SPECIFY OTHER.

General_Information

IS FARMING YOUR MAIN PROFESSION? *

- ◯ Yes
- ◯ No

HOW MANY BAGS OF CROP YIELDS DO YOU NORMALLY PRODUCE PER YEAR? *
number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)

HOW MANY BAGS OF YOUR PRODUCTION DO YOU SELL? *
number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)

AND THAT IS HOW MUCH INCOME PER YEAR FROM SELLING FARMING YIELDS? *
income from the WHOLE year or season in NAD

DOES YOUR HOUSEHOLD RECEIVE ANY REMITTANCES FROM PEOPLE WORKING ELSEWHERE? (E.G. IN RUNDU) *

- ◯ Yes
- ◯ No

IS YOUR HOUSEHOLD OR SOMEONE IN YOUR HOUSEHOLD RECEIVING ANY PENSIONS (E.G. OLD OR HANDICAPPED) *

- ◯ Yes
- ◯ No

TOTAL INCOME INCLUDING EVERYTHING YEARLY? IN NAD *
income from: farming, remittances, pensions and other income

5/13

WHICH OF THE FOLLOWING ASSETS DOES YOUR HOUSEHOLD OWN? *

- ☐ electricity from powerline
- ☐ generator for electricity
- ☐ radio
- ☐ television
- ☐ refrigerator
- ☐ motorized vehicle
- ☐ bicycle
- ☐ none
- ☐ phone

WHAT MATERIAL IS YOUR HOUSE MADE OF? *

- ◯ tent
- ◯ reed house (nsugo sonombu)
- ◯ timber and termite mud
- ◯ corrugated iron
- ◯ stone
- ◯ bricks

Agriculture

IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WERE GOOD HARVEST YIELDS? *

- ☐ 2007
- ☐ 2008
- ☐ 2009
- ☐ 2010
- ☐ 2011
- ☐ 2012
- ☐ 2013
- ☐ 2014
- ☐ 2015
- ☐ 2016
- ☐ 2017
- ☐ none
- ☐ don't know

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IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WERE BAD HARVEST YIELDS? *

- [ ] 2007
- [ ] 2008
- [ ] 2009
- [ ] 2010
- [ ] 2011
- [ ] 2012
- [ ] 2013
- [ ] 2014
- [ ] 2015
- [ ] 2016
- [ ] 2017
- [ ] none
- [ ] don't know

WHAT TYPE OF CROPS DO YOU CULTIVATE? (MORE THAN ONE POSSIBLE!) *

- [ ] Maize
- [ ] Mahangu
- [ ] Sorghum
- [ ] Vegetables
- [ ] Other

SPECIFY OTHER.

HOW MANY CATTLE DOES YOUR HOUSEHOLD OWN? *

DO YOU USE FERTILIZERS FOR CULTIVATION? *

- ◯ Yes
- ◯ No

Land_tenure

HOW MANY HECTARES OF FIELD DO YOU CULTIVATE *

one hectare = 100 x 100 meters = one large football field

AND THAT IS HOW MANY FIELDS? *

DID YOU LEAVE ANY FIELDS FALLOW IN THE LAST 5 YEARS? *

- ◯ Yes
- ◯ No

7/13

DO YOU PLAN TO USE THEM AGAIN ONE DAY? *

- ◯ Yes
- ◯ No

ARE YOU CUTIVATING A FIELD THIS YEAR THAT YOU HAD LEFT FALLOW FOR SOME YEARS IN THE PAST BUT THEN RETURNED TO AGAIN?

- ◯ Yes
- ◯ No

FOR HOW LONG HAVE YOU BEEN USING THE LAND YOU CULTIVATE NOW? IN YEARS

DO YOU HAVE ANY PLANS TO CHANGE LANDS OR EXPAND YOUR CULTIVATION AREA IN THE NEXT FIVE YEARS? *

- ◯ Yes
- ◯ No

WOULD YOU SAY THERE IS SUFFICIENT LAND FOR EVERYONE? *

- ◯ Yes
- ◯ No

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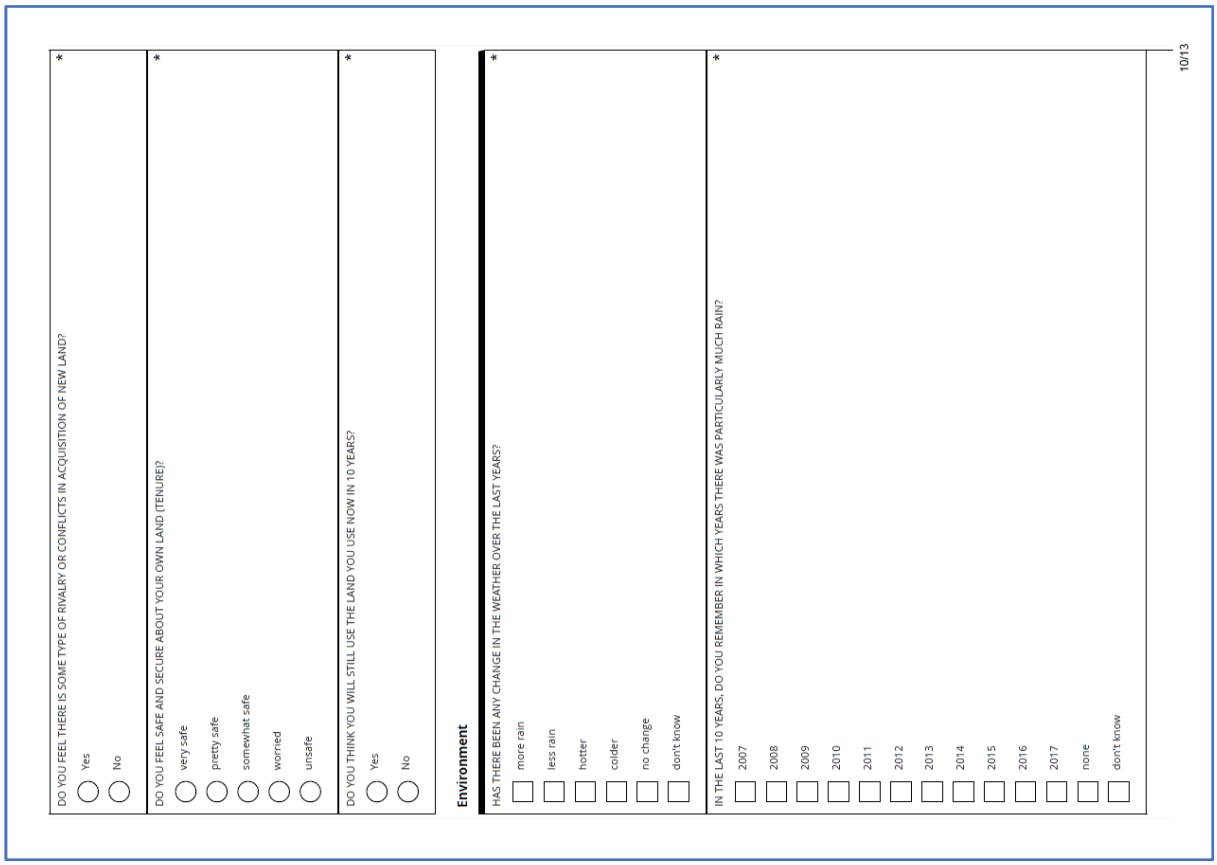

DO YOU FEEL THERE IS SOME TYPE OF RIVALRY OR CONFLICTS IN ACQUISITION OF NEW LAND? *
Yes
No

DO YOU FEEL SAFE AND SECURE ABOUT YOUR OWN LAND (TENURE)? *
very safe
pretty safe
somewhat safe
worried
unsafe

DO YOU THINK YOU WILL STILL USE THE LAND YOU USE NOW IN 10 YEARS? *
Yes
No

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Environment

HAS THERE BEEN ANY CHANGE IN THE WEATHER OVER THE LAST YEARS? *
more rain
less rain
hotter
colder
no change
don't know

IN THE LAST 10 YEARS; DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY MUCH RAIN? *
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
none
don't know

DO YOU REMEMBER IN WHICH YEARS YOU CLEARED FOREST FOR A NEW FIELD? *
none
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
never
don't know

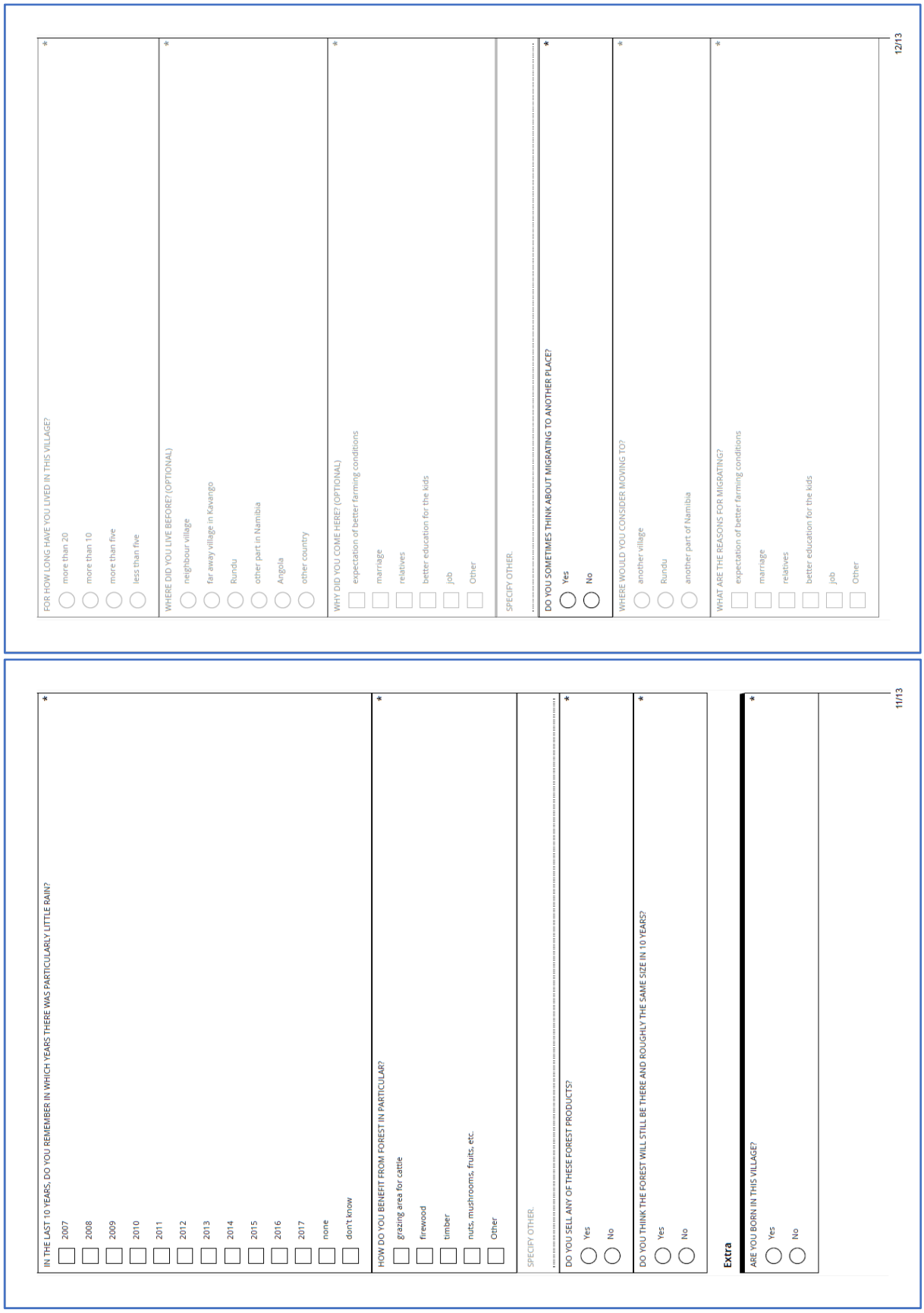
HOW MUCH ON AVERAGE WHEN YOU DID SO PER YEAR? *
(surface in hectare or meters, add unit!), put 0 if you they did not clear,
.....

DO YOU NEED PERMISSION FOR CLEARING? *
no
yes, from headman
Other

SPECIFY OTHER.
.....

HAVE YOU BEEN DENIED PERMISSION TO CLEAR IN THE LAST YEARS? *
Yes
No

9/13



FOR HOW LONG HAVE YOU LIVED IN THIS VILLAGE? *
- more than 20
- more than 10
- more than five
- less than five

WHERE DID YOU LIVE BEFORE? (OPTIONAL) *
- neighbour village
- far away village in Kavango
- Rundu
- other part in Namibia
- Angola
- other country

WHY DID YOU COME HERE? (OPTIONAL) *
- expectation of better farming conditions
- marriage
- relatives
- better education for the kids
- job
- Other

SPECIFY OTHER.

DO YOU SOMETIMES THINK ABOUT MIGRATING TO ANOTHER PLACE? *
- Yes
- No

WHERE WOULD YOU CONSIDER MOVING TO? *
- another village
- Rundu
- another part of Namibia

WHAT ARE THE REASONS FOR MIGRATING? *
- expectation of better farming conditions
- marriage
- relatives
- better education for the kids
- job
- Other

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IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY LITTLE RAIN? *
- 2007
- 2008
- 2009
- 2010
- 2011
- 2012
- 2013
- 2014
- 2015
- 2016
- 2017
- none
- don't know

HOW DO YOU BENEFIT FROM FOREST IN PARTICULAR? *
- grazing area for cattle
- firewood
- timber
- nuts, mushrooms, fruits, etc.
- Other

SPECIFY OTHER.

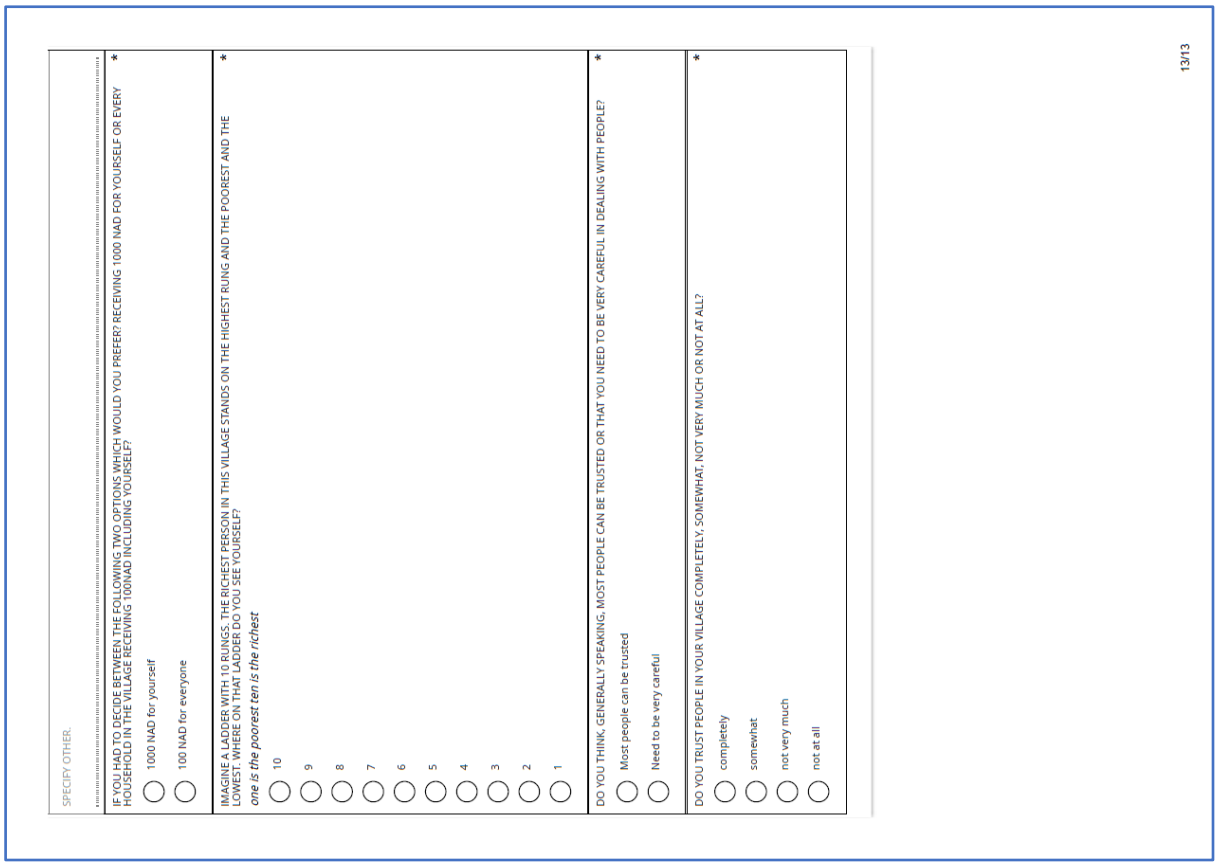
DO YOU SELL ANY OF THESE FOREST PRODUCTS? *
- Yes
- No

DO YOU THINK THE FOREST WILL STILL BE THERE AND ROUGHLY THE SAME SIZE IN 10 YEARS? *
- Yes
- No

Extra

ARE YOU BORN IN THIS VILLAGE? *
- Yes
- No

11/13



Supplement E. Information for data and analysis script request

Dataset and script for the data preparation and analysis ("do-file") can be made available upon request to the corresponding author. Game protocol and instruction are also available in the local languages spoken in Kavango, Namibia.

Chapter II:

Introducing and Terminating External Incentives: A Field-Experimental Study on Forest Conservation as a Common-Pool Resource Dilemma

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JEL-classification: C91, C93, D91, Q15, Q23, Q24, Q28, Q51, Q58

Funding and Acknowledgements

Funded by the Southern African Science Service Center for Climate Change and Adaptive Land-Use Management (SASSCAL) through the German Federal Ministry for Education and Research [grant number 01LG1201B]. Special thanks go to all participating communities for their hospitality and to all research assistants, in particular Adrian, Anastasia, Blondy, Cypriaan, James, and Moses.

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[3] The authors declare that there is no conflict of financial, general, or institutional competing interests.

Highlights:

- Common-pool resource field experiment with real resource users
- Comparing effects of different types of external incentives
- Individual rewards, collective rewards and individual fee payment incentives tested
- Heterogeneous effects after termination
- Cooperation also increased in a control group that never received incentives

Abstract

The aim of this field-experimental study is to analyze whether external institutional incentives have a lasting effect on conservation that persists even after they are terminated. We set up a forestry-framed common-pool resource game in northern Namibia and introduced individual reward and fee incentives as well as collective rewards, all of which aimed to increase cooperation. The participants (n=504) in the game were small-scale farmers and they had to make decisions about either clearing new fields in a fictional forest or staying on their old ones, which resembles decisions they make in real life. The game was played over several periods and the incentives were ceased after some time to test for lasting effects in a post-incentive period. Results show that all three types of incentives led to an improvement in conservation while implemented. Increases in cooperation persisted after termination of disincentives (fees). After termination of both types of reward incentives, on the other hand, cooperation decreased, albeit not significantly. Interestingly, cooperation increased over time in a control group that had never received additional incentives.

Keywords: common-pool resources, field experiment, deforestation, conservation interventions, conservation policy, sustainable land use

1. Introduction and Development of Hypothesis

Small-scale farmers in Sub-Saharan Africa and other regions constantly face the challenge of sustaining agricultural yields that support their livelihoods. In particular those, who live and cultivate soils where fertility rapidly decreases, will periodically move their fields to fresh, more fertile grounds, leaving their old fields fallow. In this process, new fields are usually being created by clearing surrounding forest areas. In densely populated areas this practice can result in destruction of forests and therefore scarcity of untouched land. In less densely populated areas, where forests are still abundant, there is also a strong incentive to extensively use the land resource by clearing and shifting often.[1] The conversion of forest into agriculturally used land is considered the major driver for global deforestation (Benhin 2006; Hosonuma et al. 2012; Kissinger et al. 2012). At the same time, however, intact forests also serve as an important component of livelihoods for small-scale farming households and their communities. Benefits that the environment and forest areas, in particular, provide are called "ecosystem services" (Costanza et al. 1997; Millenium Ecosystem Assessment 2005). Locally, it can be the option of collecting natural resources like foods, timber and firewood, using the forest as grazing area for their animals and, in the long run, preserving forest areas for future use (cf. Flower and Rooyen 2001; Barnes et al. 2005; Mamo et al. 2007; Luckert and Campbell 2012). On a larger scale, forests reduce the danger of desertification in dry areas, prevent land degradation such as erosion of soils and offer habitats for many species, hence preserving biodiversity (Costanza et al. 1997; Daily 1997; Grainger et al. 2000; Geist and Lambin 2004; Engel et al. 2008; Grainger et al. 2009). Globally, they contribute to carbon storage and sequestration and thereby play a crucial role in combating climate change (Gullison et al. 2007; Pan et al. 2011; Houghton et al. 2015; Griscom et al. 2017; Mitchard 2018).

Slower rates of shifting and clearing would therefore not only yield benefits to the local communities and allow more sustainable long-term land use but also contribute to a healthier global environment. Where clearing is highly destructive and unsustainable, calls for policy interventions to enforce desired levels of land use might arise. One of the most straightforward institutional interventions would be the imposition of a tax or a penalty fee for clearing forest area. Following the logic of a Pigouvian tax, negative externalities caused by deforestation would be internalized and thus leading to an efficient outcome (Pigou 1920; Baumol 1972).

[1] In our setting in Kavango, Namibia villagers do not usually entirely abandon the area and move their whole household and equipment to another place. They mostly clear and extend their fields further into the forest, adjacent to their old fields. Cultivation can in some cases also be considered as semi-permanent if farmers return to fields that had been left fallow for many years (cf. Ruthenberg et al. 1980).

Feasibility of taxation or command-and-control policies is, however, limited, particularly in regions without clear, individual land rights and meant-to-be payers living close to subsistence. Further, there is little reasonable basis for denying traditional land users and owners their rights to consume natural resources, even if this entails advancing degradation (Engel 2016). Imposing taxes or penalty payments on the poorest smallholder farmers in order to benefit the general, and possibly global, public surely raises questions about equity and fairness of such approaches (Pagiola and Platais 2006; Kemkes et al. 2010; Engel 2016). As an alternative, it has therefore been suggested and, in some cases, implemented to offer positive incentives to land users for environmentally friendly behavior (Ferraro and Kiss 2002; Wunder 2005; Wunder 2007; Engel et al. 2008; Pagiola 2008; Pattanayak et al. 2010; Muradian et al. 2013; Wunder 2013; Engel 2016). This approach is known as payments for ecosystem services (PES) and can be interpreted as a Pigouvian subsidy, as a compensation payment for missed revenues or, if environmental services are traded on markets, as a Coasean arrangement (Wunder 2005; Gullison et al. 2007; Engel et al. 2008; Vatn 2010; Engel 2016).

An increasing number of studies addresses the possibilities and challenges of designing and implementing such PES schemes for environmental conservation (Pattanayak et al. 2010; Kemkes et al 2010; Farley and Costanza 2010). As in many settings closing individual contracts with land users would entail extreme transaction costs due to small scale of cultivation areas, type of land tenure or simply due to remoteness and difficulty to monitor individual land use, considerations about collective PES programs have been made (Corbera et al. 2007; Corbera et al. 2009; Sommerville et al. 2010; van Nordwijk et al. 2012; Wunder 2013; Engel 2016; Moros et al. 2019). Kerr et al. (2014) and Hayes et al. (2019) discussed, amongst others, the challenges and feasibility of such collective PES programs. As there are only few collective payment schemes implemented in real settings yet (see e.g., Muñoz-Piña et al. 2008; Huang et al. 2009; Corbera et al. 2009; Hayes et al. 2015), scientific insights have been complemented by economic lab and field experiments, for example, by Travers et al. (2011) who found collective incentives to be more effective than individual ones. Narloch et al. (2012), on the other hand, found individual incentives to work more effectively and collective incentives to negatively affect previously existent social norms. Gatiso et al. (2018) also favors individual over collective incentives for the purpose of rising cooperation. If the conditionality of the collective reward payments is set on the performance of a group of actors, it will introduce another level of a social dilemma about receiving or not receiving the reward payments which may result in potentially ambiguous consequences for conservation behavior (cf. Engel 2016; Vollan et al. 2018)

Several studies, including experimental ones, have found adverse effects of external incentives on effort tasks and also on cooperation behavior, a phenomenon known as crowding-out: First considerations about crowding effects on social preferences can be traced back to a well-known non-experimental study about payments for blood donations by Titmuss (1970). Titmuss described how payments that were meant to encourage more people to donate blood did actually crowd out blood donors' intrinsic motivation to do so. Consequently, the total blood donations decreased. At roughly the same time, Deci (1971) conducted some small psychological experiments and observed that groups that had first received payments which were later withheld performed worse than groups that had never received payments for the same task. He interpreted that people are no longer intrinsically motivated to do a task when feeling that they should receive money for it. Later, Deci (1999) conducted a meta-study about external reward incentives and found them to potentially reduce intrinsic motivations. Similarly, Frey and Jegen (2001) as well as Bowles (2008) argue that different types of incentives or policies may affect intrinsic motivations and undermine altruism.

Another well-known example for a crowding-out of intrinsic motivation can be found in a study by Gneezy and Rustichini (2000) who observed that after the introduction of a fine for parents, who arrived late at a day-care center to pick up their children, actually more parents started coming late. The disciplinary measure did by no means foster compliance, but was understood as the fair price for being late, which consequently replaced the intrinsic responsibility for being on time. Considerations about crowding effects have also been made in the context of payments for environmental services, where pre-existing norms and motivations could be lost if conservation tasks become monetarized (Vatn 2010; Farley and Costanza 2010). In a forestry-framed field experiment, Cardenas et al. (2000) discovered that external regulations can crowd out previously existent social behavior. Vollan (2008) explains on the basis of his field-experimental observations how previously existing norms can be crowded out, particularly when external interventions are perceived as restrictive or controlling. When perceived as supportive on the other hand, they will more likely work or even lead to crowding-in.[2] This finding follows a theory described by Frey and Jegen (2001), who argue that a controlling policy

[2] To our understanding a crowding-in means that an individual's intrinsic motivation is affected positively. An increase in cooperation or conservation due to the purely economic effect of external incentives would thus not be counted as a crowding-in. Frey and Jegen (2001) describe this as a price or disciplining effect. It has a clear direction and is to be analytically separated from the effect on intrinsic motivation, which, in contrast, might shift in either direction or remain unaffected. The total effect that is observed is then the sum of both effects combined. Price and motivational effect may have the same direction and add up, or they may compete against each other with a positive or negative net outcome. Theoretically, they could even exactly cancel out each other.

may crowd out intrinsic motivation as it impairs self-determination as well as self-esteem. In other words, a confronted individual might feel reduced freedom in their decisions and also insufficient acknowledgement of their (previously) existing intrinsic motivation. Crowding-in effects on the other hand may happen if the measure is perceived as supportive, leaving freedom to decide and strengthening self-esteem in doing the right thing (Frey and Jegen 2001). A meta-study by Rode et al. (2015) gives an overview over empirical findings on crowding-effects.

As the motivation for this study, we hypothesize that an intrinsic motivation to cooperate or to conserve can be permanently crowded out by even a short period of external incentives, and thereby last beyond the actual period in which such incentives were in effective use. Then again, a period of incentivization could theoretically also lead to learning and internalization of a norm with lasting positive effects, which could then be called a crowding-in. There are a few studies that experimentally tested a similar research question in a field setting. A forestry-framed dictator game (Kaczan and Swallow 2016) and a voluntary contribution game (Kaczan et al. 2017) were conducted as field experiments in Tanzania. There was some evidence found for crowding-in effects after termination of penalty incentives in the dictator game. No significant lasting effects were found for reward incentives in both studies. Another study was conducted by Salk et al. (2017) in Lao PDR. They used a forestry-framed common-pool resource game to compare different reward incentives in a within-subject design. No lasting effects of such incentives could be found after termination.

Our research's novel contribution consists of a combination of three relevant elements: Firstly, we use a set of different incentive types that are being tested, namely collective and individual rewards as well as individual fee payments. Secondly, we employ a newly-designed common-pool resource game that describes the common-pool resource environment of the study site. And thirdly, we add a control group that does never receive any incentives so that the analysis can be conducted both within- and between-subject. We formulate our hypotheses as:

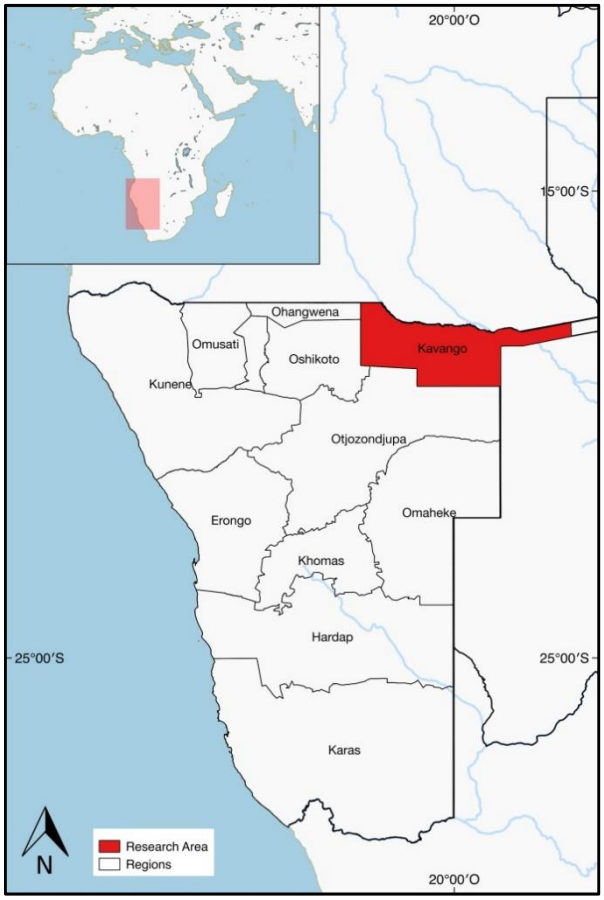
> ***H1: After the termination of external incentives, there are lasting effects on conservation***
>
> ***H0: After the termination of external incentives, there are no lasting effects on conservation***

Taking into account the possibility that incentivized periods might have either beneficial or adverse effects on future developments, results from this study could be relevant for policymakers when considering the introduction of incentive policies. This can, in particular, play a role when a potential policy can only be maintained for a limited period of time, for example due to political or financial restrictions. Additionally, further disentangling behavioral motivational effects will help advance insights on the theory of crowding.

2. Setting of the Research

The research is set in the Kavango region in northern Namibia (figure 1[3]). Unlike most of the southern, drier parts of Namibia, the Kavango area[4] is a semi-arid zone with wet and dry seasons and forests cover most of the region. Due to seasonal rainfall farmers are able to grow annual crops during the rainy season and harvest in austral autumn, after the rain has ceased.

Figure 1: Map of Namibia and research area[3]



[3] Made with Natural Earth. Free vector and raster map data @ naturalearthdata.com.
[4] The Kavango area is administratively divided into Kavango West and Kavango East.

The vast majority of the rural population is engaged in agriculture with crop farming (millet) being the primary component of their livelihood and cattle farming taking the second relevant role (Namibian Ministry of Lands and Resettlements 2015). One particular challenge in crop farming in the region is that the soil is predominantly sandy and not very nutritious for growing crop plants (Mendelsohn 2009). Depending on the exact location, farmers therefore use to leave their fields after some years of cultivation and move to fresh grounds to create new fields on unused soil, which means that they clear some part of the forest (Mendelsohn 2009; Brown 2010). Before a farmer claims a parcel of forest with the intention to clear it, the land is not privately owned. Formally, it is communal land owned by the state but traditional authorities, such as village headpersons, exercise some degree of control over the land (Mendelsohn, 2009). In many villages there are some rules established about clearing parcels in the forest in order to create fresh fields. If so, the headperson is sometimes supposed to be asked for permission to clear, but clearing is not usually denied to local villagers (Mendelsohn 2009). Mendelsohn (2009) also points out that the existing communal tenure system provokes individual interests to exploit the commonage resource to the maximum. Locally, within the village, land that is cleared and cultivated by farmers is normally considered as their property.[5] Without formal, private land rights and titles there are, however, little incentives for sustainable land use, little long-term investments into land and no possibility to use it as collateral (Mendelsohn 2009; Namibian Ministry of Lands and Resettlements 2015). This further contributes to an extensive and unsustainable style of land use.

Qualitative interviews[6], that were conducted by us prior to the experimental research, confirm that local authorities and villagers are aware of unsustainable clearing practices leading to adverse living and farming conditions in the near future. Many expect a complete disappearance of forests in their village area within the next decade. As explained before, forests provide a multitude of benefits to the communities[7]. It is estimated that a household's direct yield value from the surrounding forests is much larger than the crop value from their individual fields[8] (Pröpper et al. 2015). One hectare of recently cleared field, however, produces higher yields

[5] Sometimes farmers can make claims on forest parcels that they intend to clear in the future. These are often adjacent to their old fields. Forest areas on communal land that have not yet been cleared do, however, in general not belong to individual villagers.
[6] Qualitative interviews were conducted in 2016, several months ahead of the experimental data collection in order to identify main challenges in sustainable land use in the region and to verify the relevance of the project.
[7] Survey answers collected after the experiments show how households use forest benefits: 66% stated to use forest as grazing area for their cattle, 84% for collecting firewood, 91% for collecting timber, 91% for collecting fruits and other foods and 41% even sell forest products for profit.
[8] See also Angelsen et al. (2014) and Wunder et al. (2014) on the role and importance of livelihoods that depend on forests.

than a hectare of forest. Therefore, clearing forest and cultivating new fields is individually more profitable in the short term (Pröpper et al. 2015). Due to climate and soil quality in Kavango, regrowth of forest on fields that were left fallow again is marginal and negligible as it takes centuries to regrow to its previous state (Immanuel 2019).

We therefore interpret the forest situation as a social dilemma, which develops over time. Clearing some hectares of forest does not cause a big loss in that moment or even year, but after some years of clearing the damage done by deforestation accumulates. This circumstance is reflected in the design of the conducted common-pool resource game, where social payoffs do not happen continuously but at the end of the game. Similarly, the time lag between direct private yields and social benefits that occur later incorporates the uncertainty that villagers face, not knowing how much of the resource will be left by the other users (cf. Kramer 1989).

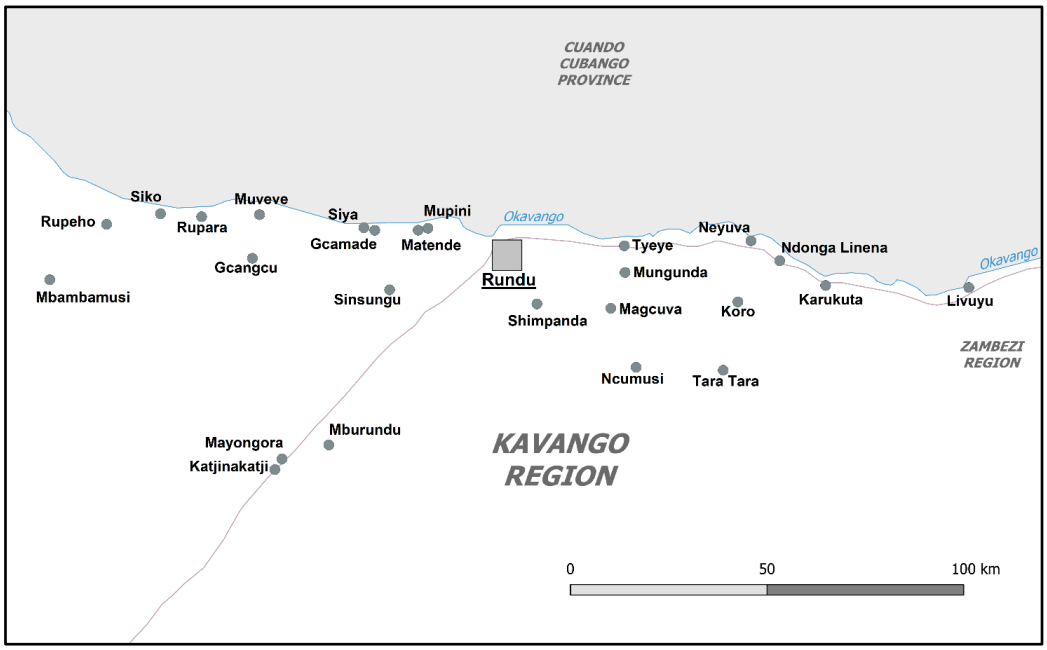
3. Data Collection

In winter 2016 (June – August) explorative interviews and surveys were conducted with Namibian officials, workers from different institutions, with traditional authorities in the Kavango region as well as with local farmers in preparation of the main research. In winter 2017 (May – August) the experiments and individual surveys were conducted in 25 randomly selected rural villages in two Kavango West districts, Kahenge and Kapako, and two Kavango East districts, Mashare and Ndiyona (figure 2[9]).

Villages too close to the urban area of Rundu were left out as the share of villagers practicing a main profession other than farming is larger. Preconditions were also that the village had more than 80 inhabitants and was not more than a day's drive away from the nearest tar road. Additionally, the sample was split in two halves in order to get an equal number of villages on both the more densely populated riverside and further inland where forests are still more abundant.

[9] Map created with "QGIS Development Team (2020). QGIS Geographic Information System. Open Source Geospatial Foundation Project. http://qgis.osgeo.org"

Figure 2: Map of research area with visited villages[9]



The total sample of this study consists of 504 participants, which amounts to 126 per treatment group. Data cleaning reduced the number of observations to 470 valid and usable ones. In the following, all tables, graphs and calculations in this paper refer to the cleaned dataset of 470 participants. Socioeconomic characteristics of the sample and a description of variables used for regression models are shown in table 1. Split samples for the experimental treatments can be found in table 2.

Table 1: Summary statistics of sample and variable info

Summary statistic of sample and explanation of variables					
	mean	sd	min	max	variable info
Groupcomp	3.41	2.45	0	6	no. of family and friends in group
Age	39.85	14.77	18	92	age of participant
Female (d)	0.60	binary	0	1	Gender (female = 1)
Schooling_years	6.07	3.90	0	19	years of schooling
Adults_in_hh	4.12	2.85	0	23	no of adults in household
Social_ladder	2.67	2.07	1	10	self-assessed socioecon. position
Village_native (d)	0.64	binary	0	1	born in the village
Farmer (d)	0.94	binary	0	1	farming as main profession
Bags_farming_yield	11.55	16.75	0	203	bags of yield produced last year
Hectares_cultivated	2.67	2.15	0	20	hectares currently cultivated
Yield_per_ha	4.95	6.31	0	60	bags of yield produced per hectare
Hectares_cleared	1.59	1.72	0	12	number of hectares cleared (real)
Clearing_rules (d)	0.62	binary	0	1	rules about clearing in village
Field_fallow (d)	0.43	binary	0	1	fields left fallow
Tenure_safety	3.64	0.60	0	4	perceived tenure security (1-4)
Observations	470				

source: own calculations based on collected data

Table 2: Summary statistics of sample by treatments

Summary statistics split by treatment	T1 Control mean	T2 Indiv.Reward mean	T3 Coll.Reward mean	T4 Indiv.Fee mean	p-value
Groupcomp	3.48	3.66	3.40	3.08	0.403
Age	40.85	38.59	39.14	40.86	0.495
Female (d)	0.65	0.59	0.56	0.61	0.567
Schooling_years	5.63	6.20	6.15	6.26	0.572
Adults_in_hh	3.96	3.97	4.10	4.43	0.205
Social_ladder	2.77	2.68	2.66	2.58	0.945
Village_native (d)	0.60	0.68	0.62	0.67	0.525
Farmer (d)	0.93	0.94	0.95	0.95	0.887
Bags_farming_yield	11.42	10.39	9.09	15.26	0.612
Hectares_cultivated	2.71	2.73	2.68	2.58	0.473
Yield_per_ha	4.63	4.94	4.30	5.91	0.379
Hectares_cleared	1.53	1.58	1.77	1.48	0.687
Clearing_rules (d)	0.58	0.59	0.59	0.72	0.089*
Field_fallow (d)	0.45	0.37	0.46	0.42	0.499
Tenure_safety	3.61	3.62	3.67	3.66	0.807
Observations	112	119	119	120	

P value for Kruskal-Wallis test or Chi2-test in case variable value is binary
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
source: own calculations based on collected data

We visited each village's headperson several days ahead of the experiments in order to arrange an appointment for a village meeting so that all villagers could be informed and invited. It was made clear beforehand that some monetary compensation would be offered for participating but also that only a certain number of people would be able to take part in the workshops.

At the beginning of the village meeting, participants would be randomly drawn by lot amongst those willing to participate, a process which was mostly perceived as fair by everyone. The same lots also determined the allocation of experimental groups. Our local, trained research assistants then explained the procedure and the instructions of the common-pool resource game, making the rules very clear, also by showing posters and giving examples for different decisions but without valuing or recommending any particular behavior. Special attention was paid to making clear that the game was not a "zero sum" situation about dividing the yields, but that cooperating actually increases the total benefits for the group as a whole. The protocols used for the village meetings, the experiment instructions as well as the survey questions can be found in the supplementary materials (C and D). [10]

Before the game was played and the particular treatment instructions were read, treatment groups were spatially divided to ensure that other groups' decisions or their treatment instructions could not influence the outcome. Our assistants also gave additional help and

[10] Protocols and instructions were translated by our assistants from English into the respective local languages and then translated back into English by another assistant in order to ensure that all translated instructions were on point. Also, all wordings and phrases used in the instructions were discussed intensively with our local assistants in preparation of the experiment as to make all instructions as clear and easily understandable as possible.

instructions to those who did not understand everything right away and those who needed help with using the tablet computers that were used for the decision making. We did, however, make sure that everybody was fit for the decision making in the real game and did not require assistance once the game started. Hence, all game decisions were made by the players individually and anonymously. Before the game started, one trial round was played, yet the result from this trial round was not made public.

After finishing the common-pool resource game, each player was asked two control questions for understanding of the game mechanics. If one or both of the control questions were answered wrongly, the player would still receive their payment but the respective observation would not be considered in the analysis.[11] Out of 504 participating individuals, 470 answered both control questions correctly, which equals 93.25%.

Next, individual surveys of about 15 minutes were conducted with each player. Payments according the participant's and their group members' decisions were done in the very end individually and in private. The whole workshop took about 4 hours in each village, out of which the experiment itself and its instruction usually took between 60 and 80 minutes. Payoffs averaged at about 80N$ (~6US$) per participant, which is more than an average local daily wage (cf. Pröpper et al. 2013). We also provided snacks and drinks to all participants during a break after the experiments, before the individual surveys were conducted.

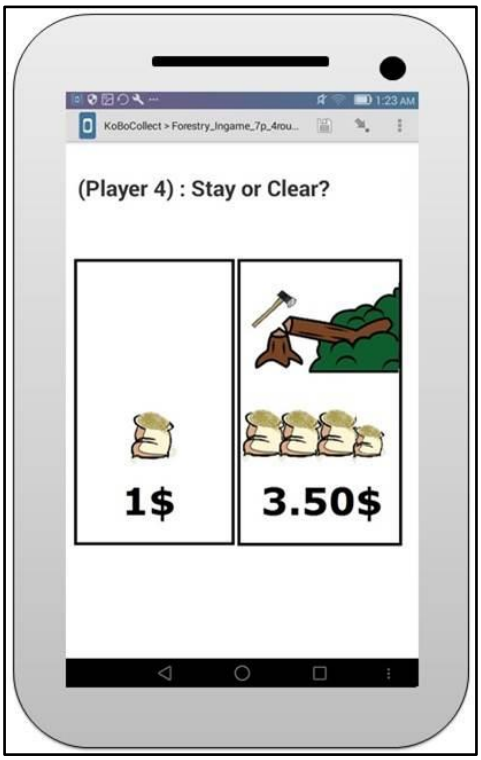
4. The Experimental Design

The forestry-framed common-pool resource experiment was specifically designed to match the given conditions in the study area and to allow experimentally testing different types of external incentives.[12] Participants earned real money according to their own and their group members' decisions. The experiment consisted of three periods of playing the common-pool resource game over four rounds each.

[11] Which led to slightly unequal sample sizes for the four treatments.

[12] It was therefore necessary to deviate from the established forestry-framed common-pool resource game as described by Cardenas et al. (2013) and Janssen et al. (2013) in favor of a design where social benefits accrue from the forest left at the end and not from regrowth rates of forest. Firstly, regrowth of forest in the Kavangos is marginal due to climatic and soil conditions. Secondly, social benefit generation based on regrowth rate of the resource could imply that a final depletion of the stock is socially optimal, which is not a desirable outcome for our study.

Figure 3: Tablet computer used for decision making in the game[14]



There were always 7 players in a group playing the game together.[13] In each of four rounds played, participants had to decide privately and anonymously whether to clear a new field or to stay on their old one (figure 3[14]). No communication between players was allowed. The socially optimal outcome was reached when everyone decided to stay on their old field only. Staying meant getting a smaller private yield of one bag, represented by 1N$, from the old field. Individually, one could earn more from clearing new fields as a fresh field yields three and a half bags, represented by 3,50N$ in the game. It was therefore individually optimal to always clear (Nash equilibrium). Each clearing of a new field did, however, decrease the size of the forest by one parcel. After every round, it was announced how many parcels had been cleared in that round by the group as a whole and how many there were left in total. It was not revealed who had cleared and who had not. Forest parcels, represented by trees in the poster (figure 4[15]) were then crossed out so that all players could assess the current stock of the forest at any time. At the end of the game, all players received their benefit from the forest which equals the

[13] Seven was chosen as the optimal group size for the experiment: If there were fewer players the marginal per capita return of cooperation would become unrealistically large. If there were more players per group, then handling of the experiment session would have become more difficult and experiments, in particular decision making, would have taken too long. Further, it is preferable, to collect data from many experimental groups due to differences in intra-group dynamics.

[14] Kobo-Toolbox was used for data collection on tablet computers during the experiment as well as for the surveys. https://www.kobotoolbox.org/ Figure 3 own illustration based on Kobo-Toolbox layout.

[15] Own illustration.

number of parcels left, i.e., 1N$ per parcel of forest. [16] The game was kept simple by asking players to decide only between the two options "staying" and "clearing" in each round. The accumulated private yields as well as the social benefits that occurred at the end added up linearly.[17] Earnings are represented by the real Namibian dollars as paid after the game[18] and decisions made with tablet computers where the two options were additionally represented by simple pictures (figure 3[14]).

Figure 4: Poster with forest parcels used in the game[15]

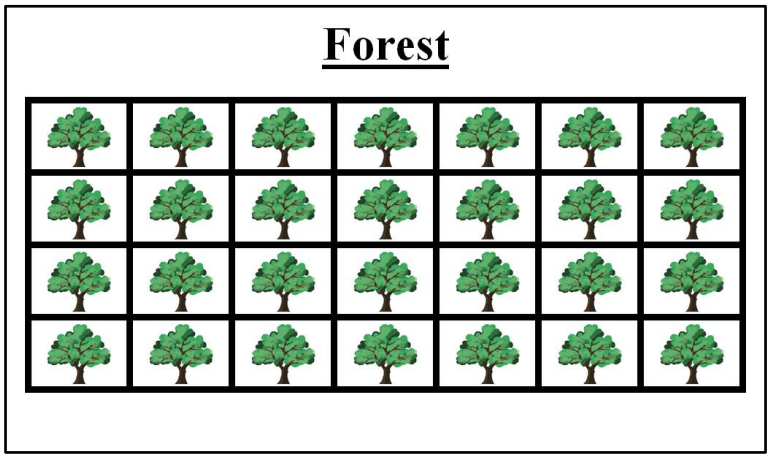


Figure 4 illustrates the stock of 28 parcels of forest. Noteworthy, there was no scarcity or competition in resource extraction: Since there were 7 players per group and the game was played over four rounds, the maximum that could be extracted if all players always cleared was 4 x 7 = 28 parcels. The forest could therefore only be depleted at the end of the very last round. If at least one player decided to stay in at least one round, there would still be forest left at the end. Consequently, no player had to worry about not being able to clear anymore towards the end of the game.

[16] The game design does not incorporate the possibility of non-linear social benefits of forest size or a threshold level, at which the forest provides a maximum of social benefits.

[17] A more complex game design over more rounds, incorporating yields decreasing non-linearly with the age of the fields, was dismissed due to comprehension difficulties during pre-tests in the field.

[18] Payoffs for staying and clearing were set after intensive pre-testing for calibration and allowing a reasonable final compensation for participating. For simplicity, we abandoned the possibility of introducing a game currency that must then be converted into real money.

Figure 5: Visualization of experimental design[19]

Period	Period 1 "Baseline"	Period 2 "Incentives"	Period 3 "Post-incentives"
Round	Round 1 2 3 4	Round 1 2 3 4	Round 1 2 3 4
Treatment 1: Control Group	no external incentives	no external incentives	no external incentives
Treatment 2: Individual Reward	no external incentives	Individual Rewards	no external incentives
Treatment 3: Collective Reward	no external incentives	Collective Rewards	no external incentives
Treatment 4 : Individual Fee	no external incentives	Individual Fees	no external incentives

The sequence of rounds and treatments is illustrated in figure 5[19]. After playing the baseline treatment in all groups for the first period, the game restarted. In the second period the control group played the same game again whereas the other groups were then confronted with different types of incentives. In the third period, all groups played the baseline game again, i.e., incentives were removed for all treatment groups. In the following paragraphs, the game structure is formalized and the single treatments explained in detail.

I. The Baseline Game (Period 1)

The experiment is set up as a dynamic common-pool resource game where the social benefit occurs at the end of the period. Since there are no discount rates used over the rounds, each decision equally affects the final private and social outcome and the personal payoff can effectively be written as the sum of all 4 rounds. The order of decisions does therefore not make any difference in economic terms. It might, however, play a behavioral role, as in the dynamic game players will learn about the other group members' decision after each round and possibly respond to that by behaving in a certain way in the following rounds.[20]

We firstly write the equation for the baseline payoff of a player in each individual round (R): Player *i* makes a dichotomous decision to either clear or not clear a new field. Let c_i be 1 when

[19] Own illustration.
[20] Players might also anticipate reciprocal behavior of others and therefore cooperate more in the early stages of the game.

player *i* decides to clear and 0 when they decide to stay. The sum "$\sum_{j=1}^{6} c_j$" at the end of the equation is the total of clearing by the other players in that round. Then:

Equation (1):

$$\underbrace{R_i(c_i, c_j)}_{\text{payoff of player } i \text{ in one round}} = \underbrace{3.50 * c_i}_{\text{yield from clearing}} + \underbrace{1 * (1 - c_i)}_{\text{yield from staying}} + \underbrace{1 * (1 - c_i)}_{\text{benefit from spared forest}} + \underbrace{1 * (6 - \sum_{j=1}^{6} c_j)}_{\text{benefit from forest spared by others}}$$

which can be reduced to

$$R_i(c_i, c_j) = 8 + 1.50 * c_i - \sum_{j=1}^{6} c_j$$

R_i = payoff player *i* in any round
c_i = decision by player *i* (0 = stay, 1 = clear)
c_j = decisions by other players *j={1,2,3,4,5,6}* (0 = stay, 1 = clear)

For the whole game over 4 rounds (r) the total payoff of a player (P) can be written as a function of all decisions made by that player and their group members. The player's total payoff (P) then equals the sum of the payoffs over all four rounds. Therefore:

Equation (2):

$$\underbrace{P_i = \sum_{r=1}^{4} R_{i,r}(c_{i,r}, c_{j,r})}_{\text{total payoff of player } i} = \underbrace{3.50 * \sum_{r=1}^{4} c_{i,r}}_{\text{yield from clearing}} + \underbrace{1 * \sum_{r=1}^{4} (1 - c_{i,r})}_{\text{yield from staying}} + \underbrace{1 * \sum_{r=1}^{4} (1 - c_{i,r})}_{\text{benefit from spared forest}} + \underbrace{1 * \sum_{r=1}^{4} \left((6 - \sum_{j=1}^{6} c_{j,r}) \right)}_{\text{benefit from forest spared by others}}$$

which can be reduced to

$$P_i(c_{i,r}, c_{j,r}) = 32 + 1.50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4} \sum_{j=1}^{6} c_{j,r}$$

P_i = total payoff of player *i* in the game (= period)
$c_{i,r}$ = decision by player *i* (0 = stay, 1 = clear) in round *r*
$c_{j,r}$ = decisions by other players *j={1,2,3,4,5,6},* (0 = stay, 1 = clear) in round *r*
r = round = *{1, 2, 3, 4}*

As $c_{i,r}$ is now the only remaining variable that player *i* can manipulate, it becomes clear that in each round clearing increases their individual payoff and also for the whole game it is individually optimal to clear a new field in every round. For the group as a whole, however, the total payoffs are maximized when no player clears any new field, which can be seen by setting $c_{i,r} = c_{j,r}$:

Equation (3):

$$\boldsymbol{S(c_r) = 7 * [\,32 + 1.50 * \sum_{r=1}^{4} c_r - 6 * \sum_{r=1}^{4} c_r\,]}$$

S = social payoff of the group, i.e., of all players combined
c_r = decision by each player in round *r* (0 = stay, 1 = clear)
r = round = *{1, 2, 3, 4}*

then,

$$\boldsymbol{S(c_r) = 7 * [\,32 - 4.50 * \sum_{r=1}^{4} c_r\,]}$$

$$\boldsymbol{S(c_r) = 224 - 31,5 * \sum_{r=1}^{4} c_r}$$

As clearing in each round linearly reduces the social payoff, the maximum is found at $c_r = 0$, i.e., no clearing. In this case the social benefit is 224 N$ which equals 32N$ per player per period. If all players act entirely selfish and always clear, the social payoff sums up to only 98N$, which is 14N$ per player.

II. The Incentivization (Period 2)

After the end of round four the game restarted. For the second period of the game, the game was therefore starting with a new forest stock of 28 parcels but now the incentive treatments were introduced. There were one control and three incentive treatments (figure 5).

- Treatment 1: Control Group

In the control treatment players played the same game as in the first period, i.e., without receiving any additional incentives. The payoff equation is therefore the same as in the baseline game of period 1 and can be written per round as equation (4a):

$$\boldsymbol{R_i(c_i, c_j) = 8 + 1.50 * c_i - \sum_{j=1}^{6} c_j}$$

and over all four rounds as equation (4b):

$$\boldsymbol{P_i(c_{i,r}, c_{j,r}) = 32 + 1.50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}}$$

- Treatment 2: Individual Reward

In the individual reward treatment, players were receiving an individual bonus of 1N$ for each round of not clearing. For this treatment, payoffs per round can be written as equation (5a):

$$\boldsymbol{R_i(c_i, c_j) = 3.50 * c_i + 1 * (1 - c_i) + 1 * (1 - c_i) + 1 * \left(6 - \sum_{j=1}^{6} c_j\right) + [\text{Incentive}: 1 * (1 - c_i)]}$$

which can be reduced to

$$\boldsymbol{R_i(c_i, c_j) = 9 + 0.50 * c_i - \sum_{j=1}^{6} c_j}$$

over all four rounds this can be written as equation (5b):

$$\boldsymbol{P_i(c_{i,r}, c_{j,r}) = 36 + 0,50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}}$$

- Treatment 3: Collective Reward

All players in the group were receiving a bonus payment of 0.50N$ in each round, as long as the total forest stock remained larger than 50% of the initial forest, i.e., larger than or equal to 14 parcels[21]. Payoffs per round are therefore given by equation (6a):

$$\boldsymbol{R_i(c_i, c_j) = 3.50 * c_i + 1 * (1 - c_i) + 1 * (1 - c_i) + 1 * \left(6 - \sum_{j=1}^{6} c_j\right) + [\text{Incentive}: 0.50\ if\ forest \geq 14]}$$

which can be reduced to

$$\boldsymbol{R_i(c_i, c_j) = 8 + 1.50 * c_i - \sum_{j=1}^{6} c_j\ +\ 0.50, if\ forest \geq 14}$$

over all four rounds this can be written as equation (6b):

$$\boldsymbol{P_i(c_{i,r}, c_{j,r}) = 32 + 0,50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r} + 0,50 * \sum_{r=1}^{4} w_r}$$

with $\boldsymbol{w_r}$ = 1 if stock remains $\geq$**14**

= 0 otherwise

[21] The bonus payment was set to only 0.50$ per round in order to achieve a comparable final outcome as the other treatments. The collective rewards are always paid in the first two rounds, as it is not possible for the group to clear more than 14 parcels of forest after two rounds.

- Treatment 4: Individual Fee

In this treatment players had to pay a fee of 1N$ for clearing forest in the respective round. The fee was not framed specifically as a punishment, penalty or tax, but was just neutrally called a fee.[22] Consequently, payoffs per round are as in equation (7a):

$$\boldsymbol{R_i(c_i, c_j) = 3.50 * c_i + 1 * (1 - c_i) + 1 * (1 - c_i) + 1 * \left(6 - \sum_{j=1}^{6} c_j\right) - [\text{Incentive}: 1 * c_i]}$$

which can be reduced to

$$\boldsymbol{R_i(c_i, c_j) = 8 + 0.50 * c_i - \sum_{j=1}^{6} c_j}$$

over all four rounds this can be written as equation (7b):

$$\boldsymbol{P_i(c_{i,r}, c_{j,r}) = 32 + 0,50 * \sum_{r=1}^{4} c_{i,r} - \sum_{r=1}^{4}\sum_{j=1}^{6} c_{j,r}}$$

It is important to note that all incentive treatments do not change the individually or socially optimal decisions. It remains individually optimal to always clear and socially optimal to never clear. The gains from clearing are just smaller now than in the baseline game. The individual reward and the individual fee treatment are directly comparable against each other as they both reduce the gains from clearing by 1N$. The collective reward changes the structure of the game as it then depends on the current remaining size of forest, and thereby on the decisions of all group members and not just one player, whether the rewards can be achieved in the respective round or not.

III.The post-incentive period (Period 3)

After the end of the last round in the second period, players of the incentive treatments were informed that for the next period the incentives were going to be removed again. For this third period, all players thus once more played the game without incentives, like in the first period. In order to avoid end-game effects, players were never told how many periods, i.e., repetitions of the game, they were going to play in total. Clearing behavior in this third period will be particularly relevant and compared to both the first period, before incentives were introduced, as well as the second period, when conserving behavior was incentivized externally.

[22] The reason for selecting the rather neutral term "fee" is based on the consideration of trying to make each treatment's framing as comparable as possible. We believed that a stronger wording as "punishment" or "penalty" sounds overly deterring, possibly creating a demand-effect in participants' behavior. Calling it a "tax" on the other hand shifts framing towards a market-based approach; understanding the fee as a "fair price" for clearing is also not exactly the treatment's intention. In conclusion, "fee" was selected as the most adequate equivalent to "rewards".

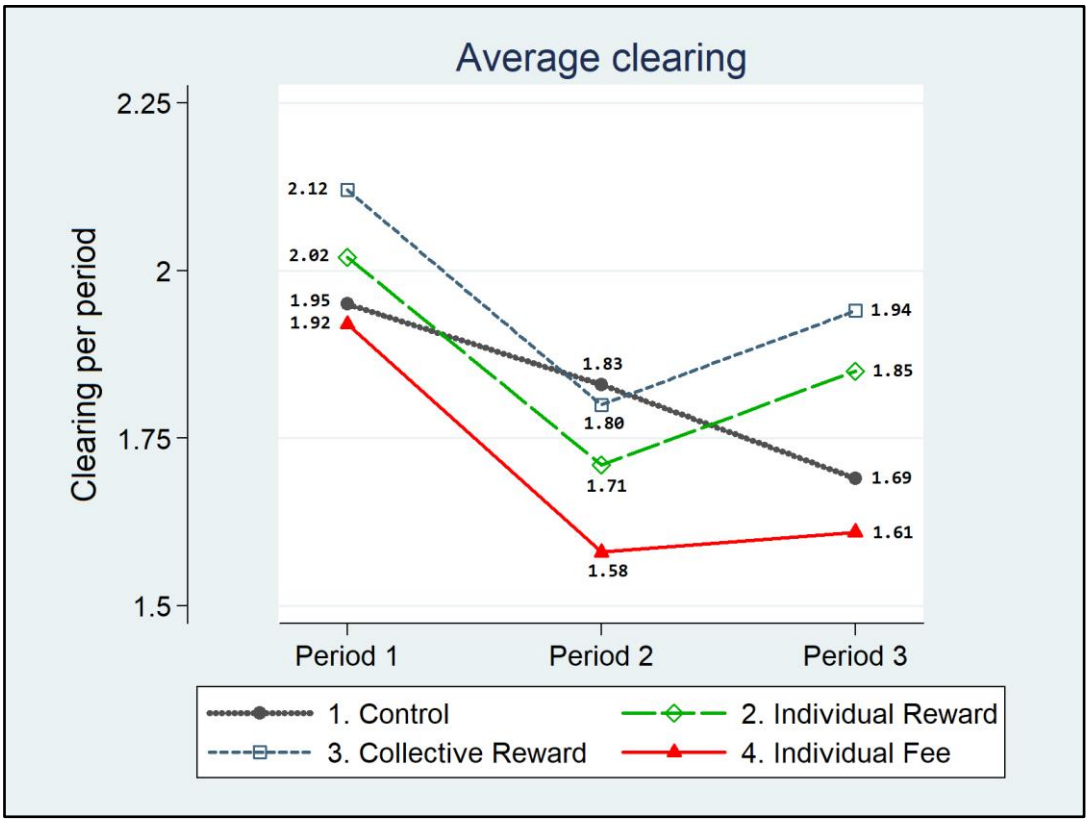
5. Analysis Method

Before analyzing possible lasting effects in period 3 it should be determined if the different types of incentives are effective in reducing clearing rates during period 2. Generally, crowding-out effects could already occur during the incentivized period and antagonize or even outweigh the intended effects of external incentives. Clearing outcomes are measured as the individual clearing per period, i.e., how many parcels a player clears on average in all 4 rounds of a period. In the next chapter they are descriptively presented in a graph (figure 6) that shows the average clearing rates over all treatments and periods. For the inferential analysis, the selected experimental design (figure 5) allows comparing the effectiveness of incentives as well as potential lasting effects from two perspectives: Firstly, as a within-subject design where the same player's change in behavior from one period to another is analyzed. This is done by single Wilcoxon signed-rank tests (table 3). Secondly, differences in clearing between the treatments can be compared in each period. Such a between-subject design is, however, susceptible to being distorted by initial variations and does not incorporate potential time trends.[23] As a better measure, we therefore compare the changes in clearing between periods, which follows the logic of a difference-in-differences approach: Changes in clearing from one period to another serve as the dependent variables for an ordinary-least-squares (OLS) regression model with robust standard errors. For facilitated comparability, differences are additionally reported in per cent, but statistical tests and regression models always rely on comparing absolute numbers. Relative (percentual) differences in the analysis would result in extremely inflated values if clearing changes from or to zero in one or another period and the regression results according to the ordinary-least-squared model would not be an adequate measure. Tests for multicollinearity of the regression models as well as tests for normality and heteroscedasticity of standard errors can be found in the supplementary materials (B).

[23] Period-wise between-subject regressions can be found in the supplementary materials (A).

6. Results

Figure 6: Average clearing in each period and treatment[24]



6.1 Effect of Incentives

Figure 6[24] and table 3 show that all three types of incentives led to a significant reduction in average clearing per player from period 1 to period 2. The average reduction of 0.33 parcels (-18%) that came along with the individual fee payments as well as the average reduction of 0.31 parcels (-15%) in the individual reward payments are significant at $p < 0.01$ (table 3: Difference P1-P2). The average reduction by 0.32 parcels (-14%) as in the collective reward treatment is significant at $p < 0.05$. In the incentivized period, incentives hence worked the way they were supposed to and no immediate crowding-out occurred. In the control treatment there is also a small decrease from period 1 to period 2 but it is not significant with $p > 0.1$. Period-wise between-subject comparisons are not presented here as the initial differences in average clearings in period 1, in particular between the control group (1.95 parcels) and both reward incentive groups (2.02 and 2.12 respectively) might impede a fair between-subject comparison of effects in later periods. Instead, comparing the changes in clearing from one period to another across treatments is less susceptible to being biased by initial variations. This difference-in-differences approach is presented in table 5 for each combination of periods.

[24] Own illustration based on collected data, created with Stata 15 statistical software.

Table 3: Average clearing and within-subject comparison

	Period1 mean (std.dev)	**Period2** mean (std.dev)	**Period3** mean (std.dev)	**Difference** P1-P2 (p-value)	**Difference** P2-P3 (p-value)	**Difference** P1-P3 (p-value)
Treatment 1 (control)	1.95 (1.47)	1.83 (1.58)	1.69 (1.55)	-0.12 (0.12)	-0.14 (0.23)	-0.26*** (0.01)
Treatment 2 (indiv. reward)	2.02 (1.56)	1.71 (1.47)	1.85 (1.57)	-0.31*** (0.00)	0.14* (0.09)	-0.17 (0.19)
Treatment 3 (coll. reward)	2.12 (1.40)	1.80 (1.47)	1.94 (1.60)	-0.32** (0.01)	0.14 (0.13)	-0.18 (0.29)
Treatment 4 (indiv. fee)	1.92 (1.53)	1.58 (1.53)	1.61 (1.52)	-0.33*** (0.00)	0.03 (0.91)	-0.31*** (0.00)

Standard deviations in parentheses for mean values,
P-values in parentheses for test results according to Wilcoxon Signed-Rank tests
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculations based on collected data

Table 4: Difference-in-Differences OLS estimation

Dependent variable: Change in clearing	**Change1to2** (p-value)	**Change2to3** (p-value)	**Change1to3** (p-value)
Indiv.Reward	-0.222*	0.290**	0.068
	(0.09)	(0.03)	(0.64)
Coll.Reward	-0.207	0.265*	0.058
	(0.14)	(0.08)	(0.72)
Indiv.Fee	-0.282**	0.174	-0.108
	(0.03)	(0.19)	(0.47)
Groupcomp	-0.008	-0.008	-0.016
	(0.69)	(0.72)	(0.52)
Age	0.005	-0.002	0.003
	(0.17)	(0.57)	(0.51)
Female (d)	0.072	-0.139	-0.067
	(0.49)	(0.15)	(0.55)
Schooling_years	0.019	0.008	0.027
	(0.20)	(0.58)	(0.13)
Adults_in_hh	0.032*	-0.002	0.030*
	(0.05)	(0.91)	(0.05)
Social_ladder	-0.061**	0.047*	-0.014
	(0.01)	(0.07)	(0.65)
Village_native (d)	0.094	-0.090	0.004
	(0.38)	(0.41)	(0.98)
Farmer (d)	0.363	0.052	0.415
	(0.22)	(0.82)	(0.15)
Bags_farming_yield	0.003	-0.001	0.002
	(0.43)	(0.72)	(0.60)
Hectares_cultivated	-0.006	-0.004	-0.009
	(0.87)	(0.89)	(0.82)
Yield_per_ha	0.001	-0.005	-0.005
	(0.97)	(0.39)	(0.69)
Hectares_cleared	0.011	-0.006	0.006
	(0.73)	(0.88)	(0.90)
Clearing_rules (d)	-0.044	0.052	0.008
	(0.67)	(0.63)	(0.95)
Field_fallow (d)	-0.269**	0.128	-0.142
	(0.01)	(0.23)	(0.24)
Tenure_safety	-0.045	0.015	-0.029
	(0.66)	(0.85)	(0.79)
_cons	-0.547	-0.183	-0.730
	(0.31)	(0.72)	(0.20)
N	470.000	470.000	470.000
F	1.417	0.932	0.642
R2	0.046	0.035	0.023

Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculations based on collected data

In the regressions, treatment dummies for each type of incentive are included as explanatory variables. In the first model (Change1to2) the coefficients for all the incentive treatments assume a negative value with the coefficients for the individual reward and the individual fee being significant at $p < 0.1$ and $p < 0.05$ respectively. In accordance with the observations from the graph, the fee treatment is the most effective one with an average reduction of 0.282 parcels, followed by the individual rewards with an average reduction of 0.222 parcels. The collective rewards are slightly less effective causing a reduction of 0.207 parcels on average and the coefficient remains below any threshold significance level. Most socioeconomic and other control variables do not seem to affect how players react to the introduced incentives. Since the regression coefficients are based on the difference-in-difference method, they do not reflect the average reduction in clearing caused by each respective treatment in absolute terms, but they indicate the reduction greater than the reduction achieved by the control group.[25] Pure between-subject regression models can, in addition to the difference-in-differences model, be found in the supplementary materials (A) but do not yield much additional insight except for the fact that clearing in the experiment correlates with real-world clearing behavior, which verifies the experiment's external validity.[26]

> *Finding 1: Individual rewards, collective rewards[27] and individual fee payments are about equally effective in increasing cooperation*

6.2 Effect of removing incentives

As the immediate effect of removing external incentives, changes in average clearing from period 2 to period 3 are compared. In absolute terms, clearing increased by 0.14 parcels (+ 8%) after both reward treatments, while it remains virtually unchanged after the termination of negative incentives (+ 2%). From a within-subject perspective, only the increase in clearing after termination of the individual reward treatment is significant at $p < 0.1$ (table 3: Difference P2-P3). Interestingly however, average clearing in the control group decreased further by 0.14 parcels (- 9%) (figure 6, table 3). This development plays an important role in the difference-in-differences regression as presented in the second column of table 4 (Change2to3). According

[25] This equals the total reduction effect minus the reduction effect achieved by the control group.
[26] Both, survey questions about whether fields have been left fallow and the size of clearing in hectares show a positive and significant coefficient in all three periods. Confusion about experimental and real world behavior during the survey interviews can be ruled out, as it was explicitly asked in which years (from the year 2000 onward) the participant remembers fields being cleared by her household (see supplementary materials D).
[27] For the collective rewards, it depends on the respective method of analysis if they can be considered as effective or not. Within-subject they also lead to a highly significant reduction in clearing. According to the DID-regression however, the coefficient remains slightly below the 10% significance threshold.

to the regression, average clearing rates did not change significantly after termination of negative incentives. After termination of both reward payments on the other hand, average clearing increased significantly in comparison to the control group. The increase is slightly larger after the termination of individual rewards with 0.290 parcels (significant at $p < 0.05$) than for the collective rewards with 0.265 parcels (significant at $p < 0.1$).

Finding 2: Cooperation also increases over time in the absence of external incentives

Finding 3: Cooperation decreases after terminating individual and collective rewards

Finding 4: Cooperation remains elevated after termination of individual fee payments

6.3 Lasting effects after removing incentives in comparison to the initial situations

Comparing changes from the first period to the third period, clearing most importantly never rose above its initial level from the first period in any treatment (figure 6, table 3: Change1to3). It stands out that clearing decreased the most after the fee treatment, by 0.31 parcels (- 16%). Both rewards treatment groups also end up at a point below their initial clearing levels, with a reduction of 0.17 parcels (- 8%) for the individual rewards and 0.18 parcels (- 8%) for the collective ones. This is, however, smaller than the 0.26 parcels reduction (- 13%) that was attained by the control group.

Therefore, from a within-subject perspective, only the negative incentive and the control group show a significantly lower level of clearing in period 3 compared to period 1 at $p < 0.01$ (figure 6, table 4: Change1to3). After termination of individual and collective rewards on the other hand, average clearing rates are, despite being lower than in their respective first periods, not significantly different from them. From a difference-in-differences perspective, the changes in clearing from the first to the last period are not statistically different from the control group for any incentive treatment (table 2: Change1to3).

Finding 5: Comparing pre- to post-incentivization periods, there is a significant increase in cooperation after terminated fee payment incentives as well as in the control group without incentives

Finding 6: Increases in cooperation from the pre- to the post-incentivization period after all types of incentive schemes are not significantly different from a control group that never received additional incentives

7. Discussion and Interpretation

External incentives in our experiment were designed in a way that, after their introduction, defection (clearing) led to an only marginally higher individual payoff in comparison to the baseline game without incentives[28]. The effect found can, however, intuitively be considered as relatively small at an average reduction of about 0.3 parcels per player and period for each type of incentive treatment. With regard to the discussion about effectiveness of negative vs. positive as well as collective vs. individual incentives, differences found in our results are rather small so that no particular type can be considered superior. In either way, the decision between implementing negative or positive as well as individual or collective incentives will most probably depend on other, externally given factors in a given environment, such as the possibility of monitoring, land rights, or political and economic agendas. At least, it can be said that neither type of incentive that was tested is entirely ineffective.

While all three incentives cause a rather similar increase in cooperation while implemented, there appear to be heterogeneous lasting effects after termination. Positive effects of individual as well as collective reward payments seem to erode when terminated. After negative incentives on the other hand, cooperation stays at roughly the same level as during the incentivization. It also remains above the level that was achieved by the control group which, somewhat surprisingly, managed to increase their average cooperation over time despite never being offered any additional, external incentives to do so. There has been quite some evidence in experimental literature that cooperation does not increase but decreases over time in repeated social dilemma games (e.g., Ledyard 1995; Neugebauer et al. 2009; Fischbacher and Gächter 2010; Smith 2015). The most obvious explanation is that participants only slowly learn that defection always leads to the highest personal payoffs and subsequently adapt to it. Andreoni (1995) on the other hand argues that, rather than slowly learning about the personal benefits of free riding, frustration about failed cooperation with other participants causes the continuous decrease. Further, even those, who aim to free-ride, might initially, in early rounds, decide to cooperate in order to motivate others to do so, too (Neugebauer 2009).

While there is strong consensus for decreases in cooperation over repeated (and identical) rounds, some studies have found empirical evidence for increases in cooperation when cooperative games are restarted (Andreoni 1988; Croson 1996; Cookson 2000; Croson 2001; Croson et al. 2005; Eckel et al. 2016; Chaudhuri and Paichayontvijit 2017). What seems to be

[28] Subtracting the social return of 1N$, the surplus gained from clearing amount up to only 0,5N$ per decision in the individual reward and the individual fee treatment.

the relevant difference is that the restart is not just playing another round but is actually presented to the participants as a restart, even if technically the rules do not change. This would also apply to the control group of our experiment. Andreoni (1988) as well as Cookson (2000) interpret this as a psychological effect that stimulates a new attempt of establishing a high cooperation norm. In any case, such a restart took place in all treatment conditions and there is no reason to assume that the control group in our experiment does not correctly portray the development of cooperation without external incentives. The results at hand therefore seem to rather discourage the use of external reward incentives if they cannot be sustained long-term or are only intended as a supportive measure for a short time. The increase in clearing after the termination of reward incentives can be interpreted as a potential crowding-out caused by monetarization of the conservation task and subsequent disappointment over the lack thereof. This would confirm Deci's (1971) interpretation that individuals are no more, or at least less, intrinsically motivated to do a task when feeling that they should receive money for it. Findings from this study speak in favor of this explanation, as a crowding-out effect caused by the intervention being perceived as controlling should otherwise also be present in the negative incentive case. The latter can be expected to exert an equally if not more restricting perception on decision makers than positive incentives. Unlike the termination of rewards incentives, the termination of fee payment cannot be expected to cause disappointment amongst participants. On the contrary, if negative incentives are terminated, it could be perceived as an improvement in freedom and self-determination, and thereby even crowd-in intrinsic motivation. Under certain circumstances, lifting negative incentives or policies that are perceived as restrictive could potentially be used as a tool to crowd-in intrinsic motivation. While in our experiment negative incentives were framed rather neutrally as a "fee" that has to be paid for clearing, lasting effects might depend on how exactly they are implemented and presented. If the fee is seen as a highly controlling punishment measure, termination might more likely crowd-in intrinsic motivation due to a perceived relative increase in self-determination. If the fee is on the other hand understood as a tax or the price for taking resources, appropriation rates might shoot up after termination as the respective policy could have caused a fortified commodification and monetarization of the resource.

Our results therefore match the previous findings by Kaczan and Swallow (2016) who found some evidence for crowding-in after termination of negative incentives. They are also in line with Salk et al. (2016) and Kaczan and Swallow (2016) as well as Kaczan et al. (2017) who did not find significant lasting effects after external reward incentives.

8. Conclusion

In all conditions, including situations where no external incentives were offered, cooperation rates were found to be well above the Nash equilibrium that would be the predicted outcome if all actors were entirely selfish, rational and profit-maximizing *homines oeconomici* (cf. Mill 1836). Increases in cooperation over time were observed in groups that were never offered any external incentives, which experimentally confirms that communities are, at least in certain environments, able to sustainably self-govern their resources (cf. Ostrom 1990; Ostrom 2005). If considering the use of incentives for a short period, for example in order to spur conservation behavior or to achieve short-term goals, it should be taken into account that the particular intervention might affect existing motivations or norms in one way or another. Results at hand cannot rule out that positive incentives in particular, such as PES programs, have the potential to permanently crowd-out intrinsic motivations to cooperate or to conserve. This could be interpreted as corroborating intuitive reservations against commodifying and monetarizing the provision of ecosystem services. In order to draw concrete policy implications about the effects of external incentives for cooperation or resource conservation and possible lasting consequences after termination of such, more research is advisable with different population samples and possibly also non-experimental empirical data. Also, our experimental design tested only one period with four rounds of playing after termination of incentives. It will be worthwhile to investigate whether cooperation rates converge again over time or whether post-incentive differences are permanent. While contemplating factors that affect selfish or cooperative land use behavior, predominant drivers for advancing degradation of forests in the Kavango research area are likely also found in widespread poverty as well as rapid population growth, both of which tremendously limit the scope for individual decision making in land and resource use. In this regard, well and carefully designed PES programs may play a double role by operating as a tool for environmental conservation as well as for poverty alleviation (see e.g., Bulte et al. 2008; Milder et al. 2010).

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Supplementary Materials Chapter II

A. Between-subject regression

B. Info on regression models

B.1 Distribution of dependent variable and test for multicollinearity

B.2 Distribution of residuals

B.3 Residuals plot and test for homoscedasticity

C. Protocol for workshop

C.1 Village meeting

C.2 General instructions

C.3 Game instructions for forestry and agriculture game

C.4. Examples

C.5: Decision making and debriefing

C.6 Treatment conditions

D. Survey questions

E. Information for data and analysis script request

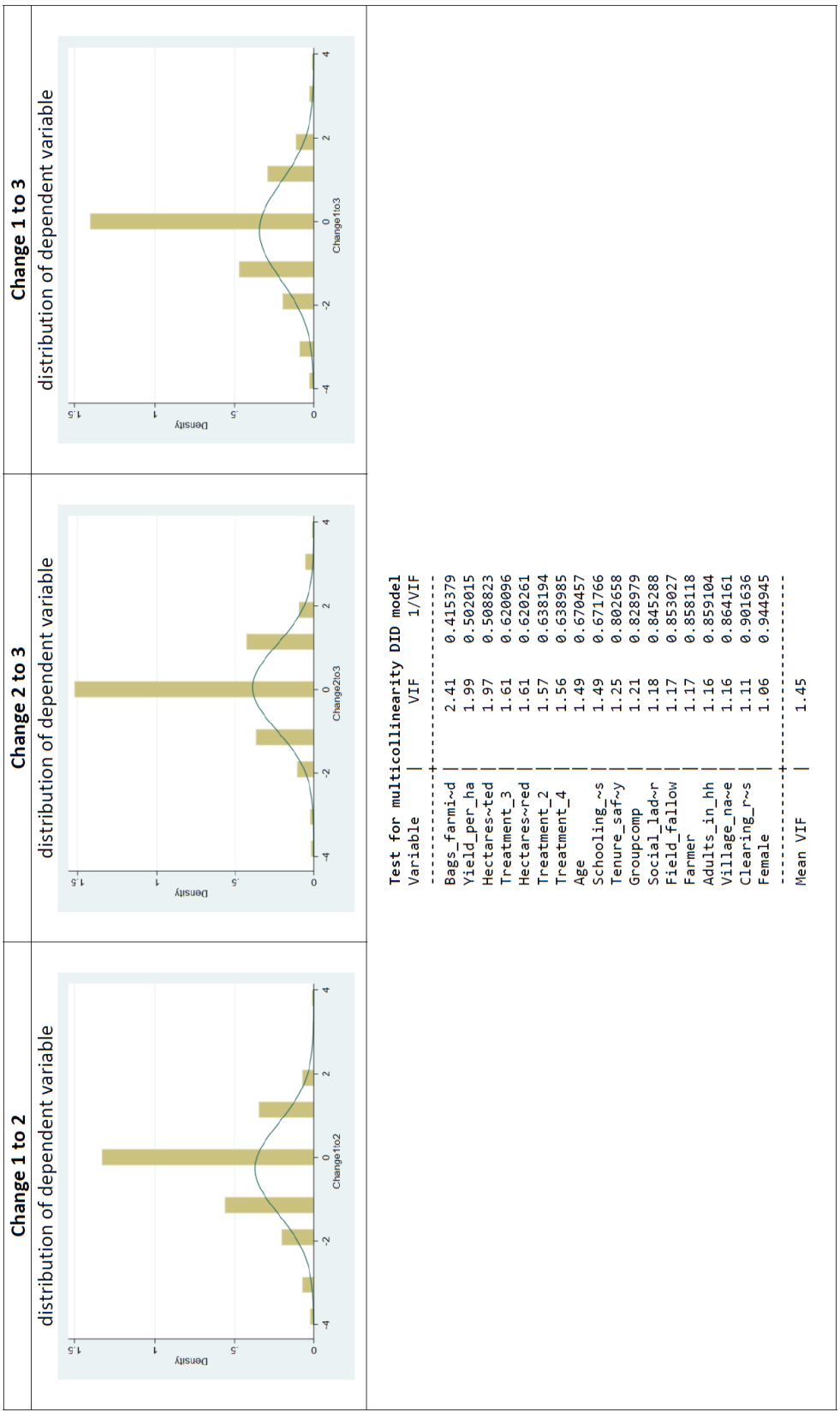
Supplement A: Between-subject regression

Dependent variable: Clearing per period	**Period1** (p-value)	**Period2** (p-value)	**Period3** (p-value)
Indiv.Reward	0.161	-0.061	0.228
	(0.39)	(0.75)	(0.25)
Coll.Reward	0.152	-0.055	0.210
	(0.41)	(0.78)	(0.30)
Indiv.Fee	0.066	-0.216	-0.042
	(0.73)	(0.28)	(0.83)
Groupcomp	-0.059*	-0.067**	-0.075**
	(0.06)	(0.03)	(0.02)
Age	0.001	0.007	0.004
	(0.81)	(0.24)	(0.45)
Female	-0.200	-0.127	-0.267*
	(0.15)	(0.37)	(0.08)
Schooling_years	-0.067***	-0.049**	-0.041*
	(0.00)	(0.02)	(0.06)
Adults_in_hh	-0.071***	-0.039	-0.041
	(0.00)	(0.11)	(0.12)
Social_ladder	0.011	-0.049	-0.003
	(0.77)	(0.17)	(0.95)
Village_native	-0.216	-0.122	-0.212
	(0.15)	(0.44)	(0.19)
Farmer	-0.417	-0.055	-0.003
	(0.14)	(0.84)	(0.99)
Bags_farming_yield	-0.004	-0.001	-0.002
	(0.51)	(0.89)	(0.74)
Hectares_cultivated	-0.003	-0.009	-0.012
	(0.95)	(0.82)	(0.76)
Yield_per_ha	-0.008	-0.007	-0.012
	(0.54)	(0.57)	(0.33)
Hectares_cleared	0.106**	0.117**	0.111**
	(0.03)	(0.02)	(0.03)
Clearing_rules	0.034	-0.010	0.042
	(0.81)	(0.95)	(0.78)
Field_fallow	0.485***	0.216	0.343**
	(0.00)	(0.16)	(0.03)
Tenure_safety	0.208*	0.163	0.179
	(0.06)	(0.19)	(0.17)
_cons	2.316***	1.769***	1.586**
	(0.00)	(0.01)	(0.02)
N	470.000	470.000	470.000
F	3.604	2.472	2.459
R2	0.110	0.082	0.086

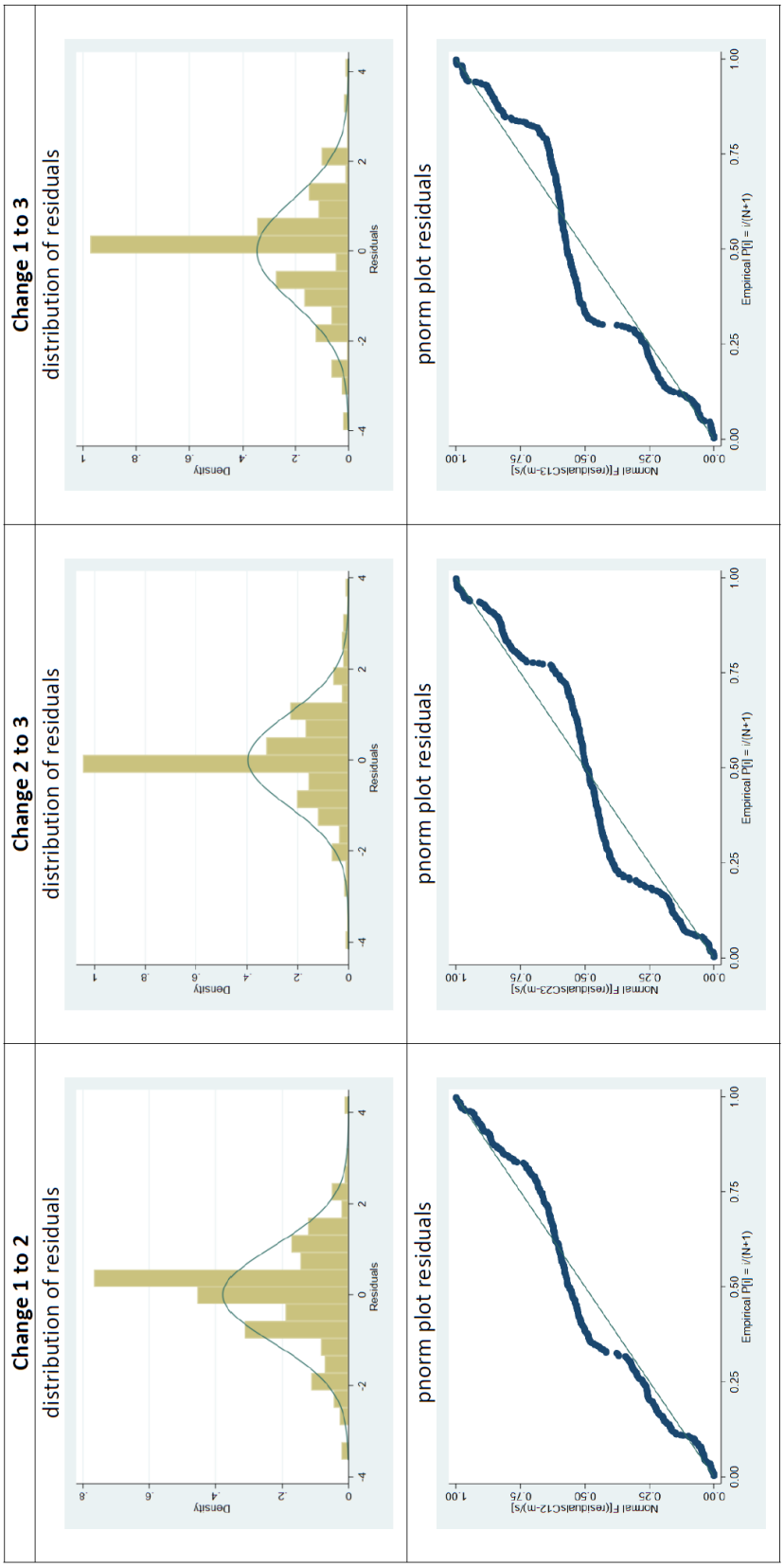
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculations based on collected data

Supplement B: Info on regression models

B.1: Distribution of dependent variable and test for multicollinearity

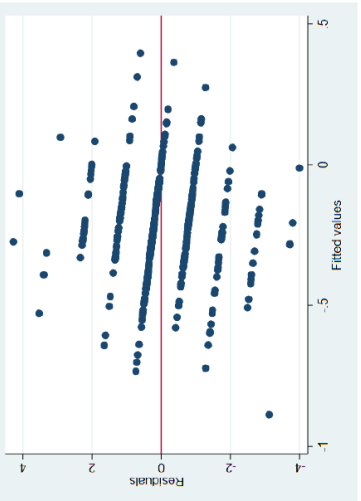


B.2: Distribution of residuals



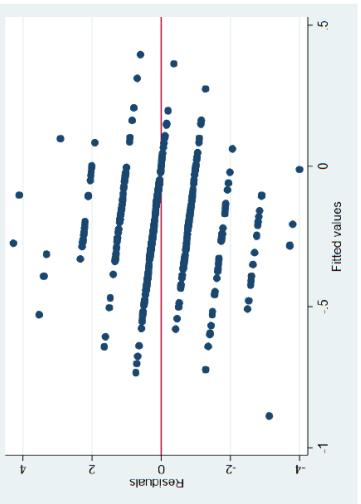
B.3: Residuals plot and test for homoscedasticity

Change 1 to 2



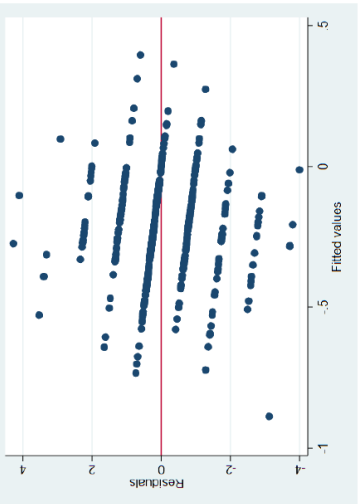

```
White's test for Ho: homoskedasticity
         against Ha: unrestricted
heteroskedasticity
         chi2(177)     =   148.76
         Prob > chi2   =   0.9398
Cameron & Trivedi's decomposition of IM-test
---------------------------------------------------
              Source |       chi2     df     p
---------------------+-----------------------------
Heteroskedasticity |     148.76    177   0.9398
          Skewness |      23.12     18   0.1860
          Kurtosis |       8.49      1   0.0036
---------------------+-----------------------------
             Total |     180.37    196   0.7816
---------------------------------------------------
```

Change 2 to 3




```
White's test for Ho: homoskedasticity
         against Ha: unrestricted
heteroskedasticity
         chi2(177)     =   176.49
         Prob > chi2   =   0.4967
Cameron & Trivedi's decomposition of IM-test
---------------------------------------------------
              Source |       chi2     df     p
---------------------+-----------------------------
Heteroskedasticity |     176.49    177   0.4967
          Skewness |      12.13     18   0.8403
          Kurtosis |      15.81      1   0.0001
---------------------+-----------------------------
             Total |     204.43    196   0.3251
---------------------------------------------------
```

Change 1 to 3




```
White's test for Ho: homoskedasticity
         against Ha: unrestricted
heteroskedasticity
         chi2(177)     =   157.47
         Prob > chi2   =   0.8516
Cameron & Trivedi's decomposition of IM-test
---------------------------------------------------
              Source |       chi2     df     p
---------------------+-----------------------------
Heteroskedasticity |     157.47    177   0.8516
          Skewness |      16.34     18   0.5687
          Kurtosis |      17.65      1   0.0000
---------------------+-----------------------------
             Total |     191.46    196   0.5783
---------------------------------------------------
```

Supplement C: Experimental workshop protocols:

C.1: Village meeting

Village Meeting

[freely presented by Christian, interpreted by Moses (assistant)]

To begin with, we would like to thank you all for coming here today. My name is Christian Hoenow. I am from the Marburg University in Germany. Together with the Ministry of Agriculture, Water and Forestry we are conducting research under the SASSCAL project. [NAME OF EXPRIMENTERS] here are also part of the project.
Doing research means we are just here to collect data, but we do not bring any type of development project into the village. What you answer in the workshop will not determine whether villages are selected for future projects. Also, we are not here to teach you anything. On the contrary, we want to learn from you. About how you do agriculture and how you use the forest. These information will help us identify potential problems and come up with possible solutions. Such solutions however will not be brought by us. We only write down what we find and then the information can later be used by the Government or by Organizations.
Today we would like to conduct a small workshop with a certain number of people. At the end of the workshop, we will also ask you several questions one by one. Unfortunately, not everyone from this village can participate since the workshop can only include a certain number of participants (28). Since we want everyone to have the same chance to participate, we have prepared a bag with as many cards as people present. Each adult that is older than 18 years may draw a card. We will ask you to fully concentrate on the workshop and we will be asking many questions. If you already know that you cannot attend for up to 5 hours, or do not wish to answer many questions, you should please not draw. Participation is, of course, voluntary!

- If you draw a red card, you will participate in the workshop, which is led by Cypriaan
- If you draw a yellow card, you will participate in the workshop led by Moses
- If you draw a blue card, you will participate in the workshop, which is led by Anastasia
- If you draw a green card, you will participate in the workshop led by Blondy
- If you draw a white card, you unfortunately cannot participate in any of the events.

Do you have any questions?
[LET EVERY ADULT DRAW A CARD]
[CONTINUE WITH GENERAL INSTRUTIONS]

C.2: General instructions

General Instructions

To begin with, thank you again for coming here today. We will conduct a workshop where you will earn real money. Different participants may receive different amounts of money. The money that you can earn is not our private money, but it is provided by the German government.
All information collected today will be used for research only. Neither the Government of Namibia or Germany nor any other organization will receive the data for other purposes. Also, neither your names nor any village-specific information will be revealed in the results. All decisions made will remain anonymous to others.
The schedule for today looks as follows:

4. We will explain the procedure of the workshop.
5. We will play small workshop like a game. This is when you can earn money.
6. After the games each of you answers a short survey-questionnaire.

It is not the purpose of the game to be better than others. Also, there are no right or wrong answers and we do not expect anything in particular from you. All payments are determined exactly the way we will explain to you later.
Before starting, I would like to give you some general information:

6. If at any time, you think that this is something that you do not wish to participate in for any reason, you are free to leave. You will however only get all money you earned if you stay until the end of the workshop.
7. If you already know that you will not be able to stay for at least 5 hours, then you should leave right away.
8. We require your complete and undistracted attention. Please, follow the instructions carefully and do not use your phone or engage in any other distracting activity.
9. It is not allowed to talk to each other during the workshop, unless we tell you to. You can ask questions after raising your hand. If you talk to each other when you are not allowed to, you will be excluded from the workshop and the payments.
10. Every one of you has received a unique player ID. Please keep this ID until the end. You must return the ID before receiving the money at the end of the workshop.

After knowing these rules, is there anybody who does not wish to participate anymore?
Do you have any questions?

C.3: Game instructions for forestry and agriculture game

[Seat groups 1-7 in blocks according to their ID color and number]
I will now explain what you will do in the workshop today. Please pay attention to what I say, as it is important that you understand everything. We have allocated you into three groups of 7 people each. I am the instructor and will tell you what the rules are, answer questions if there are any and let you know about the outcomes. We will play something similar to a game. I do not participate in the game myself. The game is played within your group of 7 people only.

In this game you will earn real money. The amount you receive depends on your decisions and the decision of other players. The numbers we mention in the game are exactly real dollar. We will note for each of you how much you earn during the game and in the end, we will give you the money in Namibian Dollars. In total you will earn between 24 and 120 Namibian Dollars at the end of the day.

It is not the purpose of the game to be better than others. There is no right or wrong, good or bad. It is possible that some players get more than other, that all players get a lot or that all players get only little. It all depends on your decisions in the game and the decisions of your group members.

The game we play is about agriculture and forest use. You will be making decisions about whether you want to clear forest in order to clear a new field or if you want to continue using your old field.
There is a forest of 28 hectares [show at board, TABLE_FOREST, 7 x 4 trees] that belongs to your group. You will play the game over 4 rounds. In the beginning the size of the forest is 28 hectares.
As you know, the soil fertility decreases over time. In the game we have old and new fields. Each player starts with an old field that will give you 1$ per round. If you decide to clear forest to get a new field, you will earn 3,50$ in that round. In every round you can decide to clear a new field. That means you clear one hectare of forest and cultivate a new field there. Thus, each clearing causes the forest stock to decrease by one hectare. A new field will give you 3,50$ only once. In the next round, the new field becomes an old field and gives you 1$, unless you clear a new field again. Clearing means you leave the old field and again get 3,50$ instead of 1$.
We are aware that in reality some of you are cultivating more than one hectare and more than one field at the same time because they have a larger household and better equipment. But for this game, every player can only cultivate one field at the same time. That means, when you decide to clear a new field, you will only be using the new field and leave the old one fallow. Also, we know that the soil fertility is slowly decreasing over time, so that in reality there are more conditions than just new and old fields.

However, we want to keep the game simple and are therefore only playing with old and new fields.
For the game this works as follows: In the first round you have to decide whether you want to clear a new field or stick to your old one. If you stay in the old one you will get 1$ in the first round. If you clear and cultivate a new field you will get 3,50$ for the first round.

Every round you make the decision to start with a new field or stay on the old one.
[**Show example for decisions:**
e.g., "Clearing twice" : 3,50 + 1 + 3,50 + 1 (1st and 3rd round)
or "Clearing three times" : 3,50 + 3,50 + 1 + 3,50 (1st, 2nd, and 4th round)]

It is possible to clear a new field in every round, which means you get 3,50$ in every round. As you see, each of you will make 4 decisions in total. Remember that there is 28hectares of forest and there are 7 players in the group. So even if everyone is clearing every round there will be 7player = 7 hectares that they can clear per round. Times 4 rounds equals 28 hectares. It is therefore not possible to deplete to whole forest before the end of the game, even if everyone is clearing new forest all the time. [FIGURE_FOREST]

So, this is the agricultural side of the game and obviously it would be reasonable to start a new field every round in order to get the highest yields from the fields. The fields are your own and therefore the yields you receive from your field are your own personal money that you can keep. There is however also another part of the game which is about the forest. You are aware that forests are not worthless but are valuable natural resources for everyone. This could for example be fruits, mushrooms, timber or firewood that you collect from the forest. Also, cattle can graze in the forest, especially during times of harvest. Finally, forests play an important role in keeping the environment healthy and sustainable. And a large remaining forest can be used by future generations for both forest benefits and leaving options for new fields. These benefits are represented in the game by a payoff that derives from the remaining forest at the end of the game. In particular, everyone will receive 1$ for every hectare of forest that is left after the end of the last round.

The number of hectares of forest left in the end depends on how much was cleared in previous rounds by you and your group members.
It is important to know that the forest benefits go to everyone. Whereas fields are owned and cultivated by one player only, the benefits from the forest at the end go to everyone equally, as nobody owns parts of the forest.
By each time you clear, you decrease the forest by one hectare. Each time you do NOT clear, it means that there will be a hectare of forest remaining which will give you and every other player 1$ in the end.

We will now do a test round for the decision making. This is not the real decision that will affect your payoffs, but it is just meant to help you understand how it works.

We will not make the decisions publicly but we will be using the tablet computers.

Therefore, all the decision you will make remain completely anonymous. The other players in your group will not find out how you decided. They will only see after each round how much forest was cleared by the group as a whole.

[Show FIGUR_FOREST and FIGURE_DECISION]

[show TABLE_DECISION].

- Remember to not show to your neighbors what you decide
- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.
- Only push the tablet **lightly** and **shortly**. We can also use this **pen**.
- The selected answer will be marked **orange**. You can still **change** your decision.
- When done, swipe **right** and **hand the tablet back** to me/assistant.

[Let Players try test round on tablet]
Let's now look at some examples (write down numbers of examples on the whiteboard! TABLE_EXAMPLES):

C.4: Examples

EXAMPLE 1:
I will use this table to make examples of how players can decide. If a player decides to stay, he/she gets 1$ **[mark].** If a player decides to clear he/she gets 3,50$ **[mark]**

[erase again]

Let's go through an example:

Imagine every player was just sticking to their old field over all rounds. Everyone then gets 1$ in each round, so 1+1+1+1 = 4$ over the 4 rounds.

In addition to that yield, there is the benefit that comes from the forest. Since nothing was cleared, the forest size remains at its initial stock of 28 hectares. For every hectare, each player receives 1 Dollars. That is 28x1 = 28 Dollars.

Remember, that the forest benefits are the same for everyone as everyone is using the same forest.

Your field on the other hand is your own and only generates benefits for yourself.
"As no one cleared, the forest size remains large and there is a lot of forest benefits for the group."

Summing up the benefit from the forest and the yield from the field, everyone receives 4 + 28 = 32 $.

Round 1	1	1	"				
Round 2	1	1	"				
Round 3	1	1	"				
Round 4	1	1	"				
SUM Yields	4	4	4	4	4	4	4

Remaining Forest = 28

SUM Total	32	32	32	32	32	32	32

EXAMPLE 2:
Let's go through another example together: Now imagine one out of the seven players decides to clear a new field in all 4 rounds. He/she will then get 3,50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$. Then the other 6 players who stay on their old field all the time still only get 1+1+1+1 = 4$ from their fields.

Since he/she cleared in every round and no one else did, the forest decreased by one hectare every round. Which is 4 hectares in total. The remaining forest in the end of the game is then 28 – 4 = 24 hectares. Remember that everyone gets 1$ per hectare of the remaining forest at the end of the game, which is 24$ for everyone.

The 6 players who did not clear do then get their agricultural yield of 4 + the forest benefit of 24 = 28$ in total.

"The one player who decided to clear every round receives 14 from agricultural yields + the same 24 from the remaining forest = 38$ in total. Which is more than what those who did not clear got.

Those other 6 player who did not clear however, just as in the previous example, now get 28 in total which is because the forest size decreased to 24 hectares from the clearing of player one"

Round 1	3.50	1	"				
Round 2	3.50	1	"				
Round 3	3.50	1	"				
Round 4	3.50	1	"				
SUM Yields	14	4	4	4	4	4	4

Remaining Forest = 24

SUM Total	38	28	28	28	28	28	28

EXAMPLE 3:
But now imagine all 7 players decide to always clear a new field in all rounds. They will then get 3,50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$ for everyone.

Since everyone cleared in every round, the forest decreased by 7 hectares every round. Which is 7*4 =28 hectares in total. There is therefore no forest remaining at the end of the game.

All players do then get their agricultural yield of 14 + but nothing from the forest, so 14$ in total.

"Note that when ALL PLAYERS decide to always clear, each players' final payoff is much smaller compared to when most of the forest stock is conserved.

If the whole group does not clear, then the group as a whole get the most. If you personally decide to clear, you always get more, but the other players in the group will get less. So, when everybody decides to clear a lot, the group benefits from the forest become smaller or vanish and in total everybody is getting less."

Round 1	3.50	3.50	"				
Round 2	3.50	3.50	"				
Round 3	3.50	3.50	"				
Round 4	3.50	3.50	"				
SUM Yields	14	14	14	14	14	14	14

Remaining Forest = 0

SUM Total	14	14	14	14	14	14	14

EXAMPLE 4:
Let's go through another example together: Now imagine one out of the seven players decides to always clear a new field. He/she will then get 3.50$ in each round. Over 4 rounds that is 3,50+3,50+3,50+3,50 = 14$. 4 Players decide to clear twice, for example in round1 and in round 3. They then get 3,50+1+3,50+1 = 9$ from their fields. The remaining 2 players only clear once and therefore get 3,50+1+1+1 = 6,50$ from their fields.
Now the forest has decreased by 4 hectares because of the clearing by the first player. By 4*2 = 8 hectares from clearing by the four players. The last two players who only cleared once caused a decrease of 2*1 = 2 hectares over all round. In total the forest size therefore decreased by 4 + 8 + 2 = 14hecatres. 28 – 14 = 14 hectares of forest that are left in the end. Remember that everyone gets 1$ per hectare of the remaining forest at the end of the game, which is 14$ for everyone.
The one player who decided to clear every round receives 16 from agricultural yields + 14 from the remaining forest = 28$ in total.

The four players who cleared twice each do then get their agricultural yield of 9 + the forest benefit of 14 = 23$ in total.

The two players who cleared once each do then get their agricultural yield of 6,50 + the forest benefit of 14 = 20,50$ in total.

"We can see that those, who clear a new field more often, get higher payoffs in the end. If however everyone decides to clear all the time, the forest benefits will vanish and everyone's final payoffs decrease" **[compare Ex.3].**

Round 1	3.50	3.50	3.50	3.50	3.50	3.50	3.50
Round 2	3.50	1	1	1	1	1	1
Round 3	3.50	3.50	3.50	3.50	3.50	1	1
Round 4	3.50	1	1	1	1	1	1
SUM Yields	14	9	9	9	9	6.50	6.50

Remaining Forest = 14

SUM Total	28	23	23	23	23	20.50	20.50

Should we have another example?

[spatially separate groups now according to their color, set them in order 1-7]
(e.g., at different sides of a building, behind the car, under different trees etc.)

Then, before we start with the game, we would like to ask you some questions. This is just to test whether you all understood the rules of the game. Remember that during the game you are not allowed to talk to each other. If there is anything unclear please ask us now and we will explain it to you. If there is anything unclear later during the game, you can still ask questions, but you will have to raise your hand and I will come to you to help you in private. So, are there any questions right now?

[**ask and answer if so, give another example if requested]**

1. **The more hectares of forest the group has cleared, the smaller becomes the forest stock. True or False?**
   [True]

2. **The benefit you receive from the forest is the same for everyone. True or False?**
   [True]

3. **The more often I clear and start a new field, the more money I will get in the end. True or False?**
   [Always True, does not depend on what the other players do]

4. **The more often the other players clear, the less money I get in the end. True or False?**
   [True, others clearing means decreased forest and decreased forest benefits for me]

The outcome does not depend on chance or luck but only on you and your group members' decisions. We promise that we do all the calculation correctly as explained in the game instructions above and will not deceive you at any stage of the game. Also, there is absolutely no possibility of you or any other player cheating or not playing according to the rules given, as you can only decide in every round if you want to clear a new field or not. We will collect your answers using these tablet computers. They are very easy to use and we will show you how it works. You only have to press "clear" or "not clear = stay" in the screen. Here we have a picture of how it looks like [Show TABLE_DECIDE]. If anything is unclear please ask us for help and do not ask the other players. We will calculate and announce to everyone how much of the forest is left after each round. [Show TABLE_FOREST] (Our calculations are done by the computer and must therefore not be questioned). You will however not learn who cleared or who did not. You only see how much forest is remaining after each round. All the decisions you make will be kept anonymous and no one will find out what the other players did. Even after the end of the game, you are not obliged to tell anyone what you and the other players decided.

Procedure: You and your group members will make your decision in each round.. Here [point TABLE_DECISION] you have to press whether you wish to stay on your old field or start a new one in every round. You are not allowed to talk to each other while the others make their decisions and while we calculate the results. After each round we will tell you how many forest parcels are left. After the last round (which is after four rounds) the game ends. You will again be informed about the final forest size, which equals the benefits that everyone receives from the forest. It is however not the end of today's workshop. We will afterwards continue with another game.

Remember, that you must not talk to each other and your decisions will remain anonymous.

ARE THERE ANY QUESTIONS?

Then let us now start with the game decisions. We start with the first round out of four and each of you can please make his/her decision on the tablet. [show TABLE_DECISION].

- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.
- Then swipe **right** and **hand the tablet back** to me.
- Remember to not show to your neighbors what you decide

[make decisions]

Thank you. This was the first round. [YY] hectares of forest were cleared. The remaining forest is [XX] hectares [mark on TABLE_FOREST]

C.5: Decision making and debriefing

We continue with the 2nd / 3rd / last (4th) round out of four and each of you please make his/her decision on the tablet. [show TABLE_DECISION].

- Press the **left** side if you wish to stay on your **old field**.
- Press the **right** side if you wish to clear a **new field**.

[make decisions]
[YY] hectares of forest were cleared. The remaining forest is [XX] hectares [mark on TABLE_FOREST]

Thank you for playing. The final remaining forest size is XX hectares. That means everyone will receive XX dollar from the remaining forest in addition to their agirulcutral yields.

Has anything been unclear during the first game? If so please ask us now. We will now continue with another game that works very similarly.

[REPEAT GAME 3x]:

[Period 1: Baseline]

[Period 2: ---See treatment conditions!---]

[Period 3:] We will now play the same game again. That means we start again with a forest stock of 28 hectares. The decisions, payoffs and results from the first game do in no way influence this second game. However, the rule we introduced in the previous session is abolished again [only if treated, nothing abolished for control group]. *(Note: This period not relevant for Scarcity Experiment)*

Debriefing and Control Questions

[AFTER ALL HAVE MADE THEIR DECISION]

Then, before we have a break, we also ask you to answer **two short questions** for understanding individually. This is just meant to help us as feedback to check if everyone understood the game.

[go to experimenter individually, try to keep groups separated so that previous players cannot go back to talk to others**]**

1. Imagine you clear a new field once and everyone else in the group clears a new field three times. Who will get a higher payoff in the end? You, the others, or same for everyone?

2. Imagine you clear a new field two times and everyone else in the group never clears a new field. Who will get a higher payoff in the end? You, the others, or same for everyone?

This is now the end of the first part of the workshop. You are now allowed to talk to each other again.

We will have some snacks and drinks and afterwards continue with a short survey-questionnaire that each of you please answer one after another. When that is done, we will do the payments and we are done.

C.6: Treatment conditions

[Treatment 1: Control Group]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.

[Treatment 2: Individual Rewards]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.
There is however an addition to the game that we will have:
In each round that you decide NOT to clear forest, you will receive 1$ as an additional reward

Let's look at an example:

Example:
Imagine a player was clearing a new field in round 1 and in round 3. He/she then gets 3,50+1+3,50+1 = 9$ over the 4 rounds from his/her fields. And he/she gets 1$ Reward in Round 2 and Round 4, when he/she did not clear, which is 2$ that get added to the 9 $

In the end, in addition to that yield and the reward, there is still the benefit that comes from the forest which is left after the end of round 4.

Another player who cleared in all 4 rounds, does not get any reward. He/she gets 3,50+3,50+3,50+3,50 = 14$ from his/her yields + 0$ Reward

A player who never clears however gets 1$ reward for every round, which is 4 in total. He/she gets 1+1+1+1 = 4$ from the yields and 1+1+1+1 = 4$ from reward, total = 8$

Round 1	3.50	3.50	1+1	1+1	1+1	1+1	1+1
Round 2	1+1	3.50	1+1	1+1	1+1	1+1	1+1
Round 3	3.50	3.50	1+1	1+1	1+1	1+1	1+1
Round 4	1+1	3.50	1+1	1+1	1+1	1+1	1+1
SUM Yields	9+2	14+0	4+4	4+4	4+4	4+4	4+4

Remaining Forest = 22

SUM Total	31+2	36	26+4	26+4	26+4	26+4	26+4

[Treatment 3: Collective Rewards]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.
There is however an addition to the game that we will have:

As a group, you will receive an additional reward in each round when there are at least 14 hectares of forest left. If 14 or more hectares of forest are left, everyone will receive 0,50$ reward per round additionally to what he/she gets from the fields and from the forest.

Let's look at an example:

Example:
Imagine again the following situation [See table]. Now, if there is at least 14 hectares of forest left, after round 1, everyone receives 0,5$ additionally. It does not depend on who cleared how much as long as the whole forest remains at 14 or more hectares.
If the remaining forest in a round is less than 14, then no one would get the extra reward.

Round 1	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5
Round 2	3.5+0.5	3.5+0.5	3.5+0.5	3.5+0.5	1+0.5	1+0.5	1+0.5
Round 3	3.5	3.5	3.5	3.5	3.5	1	1
Round 4	3.5	1	1	1	1	1	1
SUM Yields	14+1	9+1	9+1	9+1	9+1	6,50+1	6,50+1

Remaining Forest = 11

SUM total	25+1	20+1	20+1	20+1	20+1	17.5+1	17.5+1

[Treatment 4: Individual Fee]

Thank you for your decisions. We will now play the game again. Again for 4 rounds and starting with a fresh forest of 28 hectares. All payoffs are as before and you will later get the real money for both the first session that you just played and also for the next sessions that we will be playing now. You will still get 1$ in each round for staying on an old field and 3.50$ for a round when you decide to clear a fresh one. Everyone also still receives 1$ for each tree that remains in the end.
There is however an addition to the game that we will have:

In each round that you decide to clear forest, you have to pay 1$ for doing so.

Let's look at an example:

Example:
Imagine a player was clearing a new field in round 1 and in round 3. He/she then gets 3.50 +1+3.50 +1 = 9$ over the 4 rounds from his/her fields. But he/she has to pay 1$ for clearing in round 1 and in round 3. So, in total he/she gets 9 – 2$ = 7$

In the end, in addition to that yield and the reward, there is still the benefit that comes from the forest which is left after the end of round 4.

Another player who cleared in all 4 rounds, has to pay 1$ in every round for doing so. He/she gets 3.50 +3.50+3.50 +3.50 = 14$ from his/her yields - 4$ payment for clearing = 10$

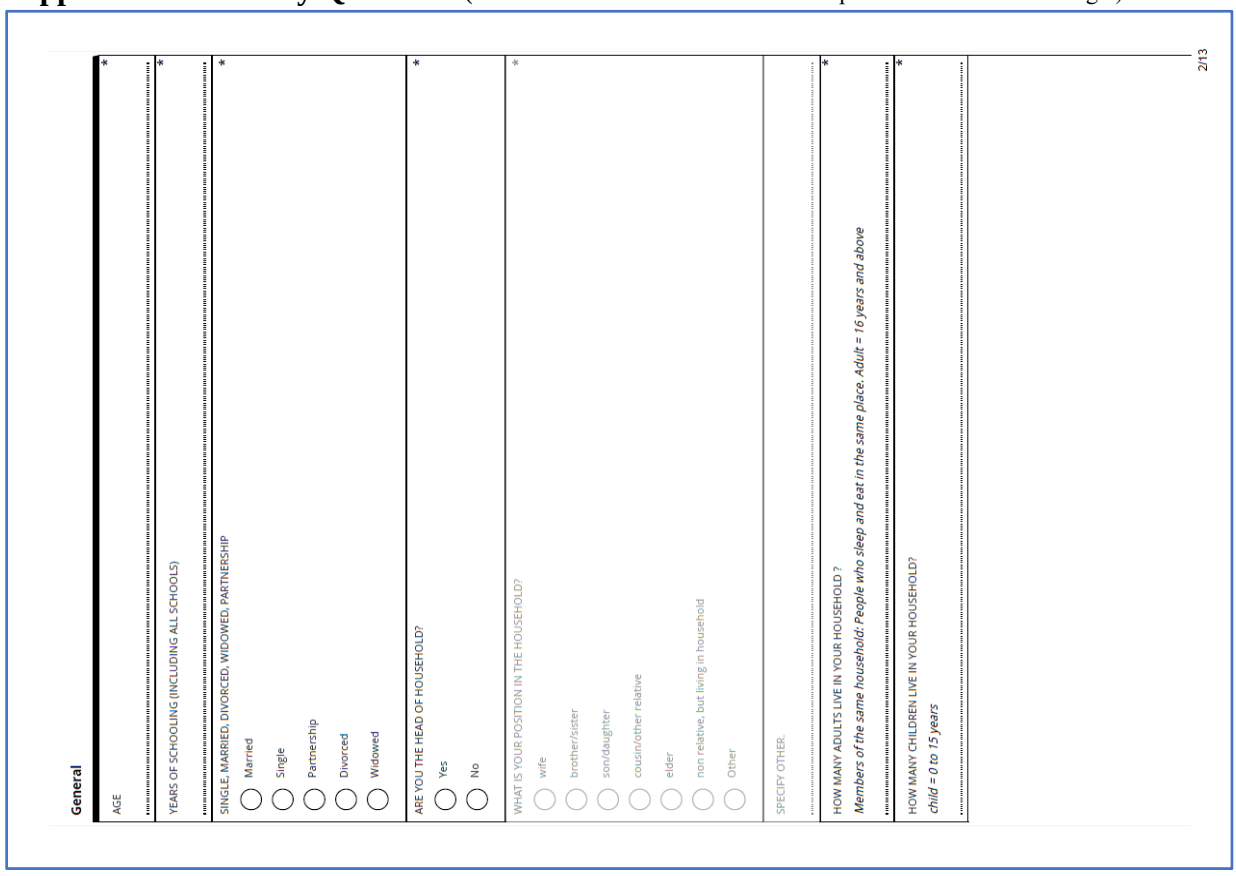
A player who never clears however does not have to pay anything. He/she gets 1+1+1+1 = 4$ from the yields.

Round 1	3.50-1	3.50-1	1	1	1	1	1
Round 2	1	3.50-1	1	1	1	1	1
Round 3	3.50-1	3.50-1	1	1	1	1	1
Round 4	1	3.50-1	1	1	1	1	1
SUM Yields	9-2	14-4	4	4	4	4	4

Remaining Forest = 22

SUM Total	31-2	36-4	26	26	26	26	26

Supplement D: Survey Questions (Sheets made with Kobo-Toolbox: "https://www.kobotoolbox.org/")



Forest SURVEY

by_enumerator

ENUMERATOR (IN THE SURVEY!)
- James
- Moses
- Anastasia
- Cypriaan
- Biondy
- Other

SPECIFY OTHER.

EXPERIMENTER (IN THE GAME!)
- James (J)
- Moses (M)
- Cypriaan (C)
- Anastasia (A)
- Bondy (B)
- Other

SPECIFY OTHER.

PLAYER ID (ONLY NUMBER!)
put the exact ID number here

TREATMENT GROUP *
- 1. Baseline
- 2. Individual Reward
- 3. Collective Reward
- 4. Individual Fee

PLAYER GENDER *
- Male
- Female
- Other / Diverse

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General

AGE *

YEARS OF SCHOOLING (INCLUDING ALL SCHOOLS) *

SINGLE, MARRIED, DIVORCED, WIDOWED, PARTNERSHIP *
- Married
- Single
- Partnership
- Divorced
- Widowed

ARE YOU THE HEAD OF HOUSEHOLD? *
- Yes
- No

WHAT IS YOUR POSITION IN THE HOUSEHOLD? *
- wife
- brother/sister
- son/daughter
- cousin/other relative
- elder
- non relative, but living in household
- Other

SPECIFY OTHER.

HOW MANY ADULTS LIVE IN YOUR HOUSEHOLD ? *
Members of the same household: People who sleep and eat in the same place. Adult = 16 years and above

HOW MANY CHILDREN LIVE IN YOUR HOUSEHOLD? *
child = 0 to 15 years

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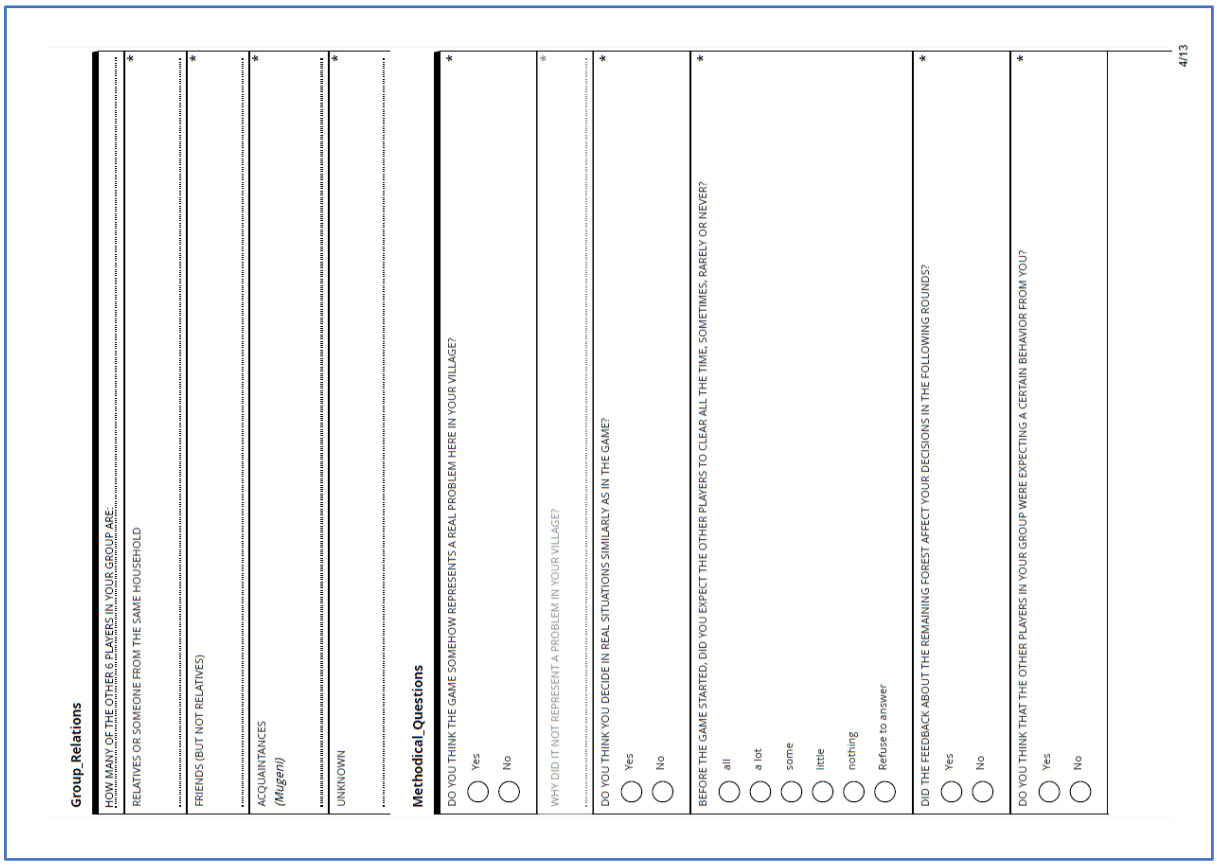

Group_Relations

HOW MANY OF THE OTHER 6 PLAYERS IN YOUR GROUP ARE: *

RELATIVES OR SOMEONE FROM THE SAME HOUSEHOLD

FRIENDS (BUT NOT RELATIVES)

ACQUAINTANCES *(Mugeni)*

UNKNOWN

4/13

Methodical_Questions

DO YOU THINK THE GAME SOMEHOW REPRESENTS A REAL PROBLEM HERE IN YOUR VILLAGE? *
- Yes
- No

WHY DID IT NOT REPRESENT A PROBLEM IN YOUR VILLAGE? *

DO YOU THINK YOU DECIDE IN REAL SITUATIONS SIMILARLY AS IN THE GAME? *
- Yes
- No

BEFORE THE GAME STARTED, DID YOU EXPECT THE OTHER PLAYERS TO CLEAR ALL THE TIME, SOMETIMES, RARELY OR NEVER? *
- all
- a lot
- some
- little
- nothing
- Refuse to answer

DID THE FEEDBACK ABOUT THE REMAINING FOREST AFFECT YOUR DECISIONS IN THE FOLLOWING ROUNDS? *
- Yes
- No

DO YOU THINK THAT THE OTHER PLAYERS IN YOUR GROUP WERE EXPECTING A CERTAIN BEHAVIOR FROM YOU? *
- Yes
- No

WHAT IS YOUR RELIGION? *
- Lutheran
- Catholic
- Traditional
- Evangelic
- None
- Don't know
- Refuse to answer
- Other

SPECIFY OTHER:

YOUR MOTHER TONGUE? *
- English
- Portugues
- RuShambyu
- RuKwangali
- RuNyemba
- Gciriku
- Mbukushu
- Kimbundu
- Chokwe
- San
- Oshiwambo
- Nama/Damara
- Afrikaans
- Otjiherero

3/13

DO YOU THINK THE PAYMENTS OFFERED ARE LITTLE, FAIR OR TOO MUCH? (REAL PAYMENTS NOT MADE YET, BUT ROUGHLY KNOW THE MIN AND MAX AMOUNT TO EARN) *

- ◯ too much
- ◯ fair
- ◯ too little

WHAT DO YOU THINK IS THE REASON FOR CONDUCTING THESE GAMES?

DO NOT READ ANSWERS

- ☐ Qualify for village support
- ☐ find out about the community
- ☐ distribute money
- ☐ don't know
- ☐ Other

SPECIFY OTHER.

General_Information

IS FARMING YOUR MAIN PROFESSION? *

- ◯ Yes
- ◯ No

HOW MANY BAGS OF CROP YIELDS DO YOU NORMALLY PRODUCE PER YEAR? *

number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)

HOW MANY BAGS OF YOUR PRODUCTION DO YOU SELL? *

number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)

AND THAT IS HOW MUCH INCOME PER YEAR FROM SELLING FARMING YIELDS? *

income from the WHOLE year or season in NAD

DOES YOUR HOUSEHOLD RECEIVE ANY REMITTANCES FROM PEOPLE WORKING ELSEWHERE? (E.G. IN RUNDU) *

- ◯ Yes
- ◯ No

IS YOUR HOUSEHOLD OR SOMEONE IN YOUR HOUSEHOLD RECEIVING ANY PENSIONS (E.G. OLD OR HANDICAPPED) *

- ◯ Yes
- ◯ No

TOTAL INCOME INCLUDING EVERYTHING YEARLY? IN NAD *

income from: farming, remittances, pensions and other income

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WHICH OF THE FOLLOWING ASSETS DOES YOUR HOUSEHOLD OWN? *

- ☐ electricity from powerline
- ☐ generator for electricity
- ☐ radio
- ☐ television
- ☐ refrigerator
- ☐ motorized vehicle
- ☐ bicycle
- ☐ none
- ☐ phone

WHAT MATERIAL IS YOUR HOUSE MADE OF? *

- ◯ tent
- ◯ reed house (nsugo sonombu)
- ◯ timber and termite mud
- ◯ corrugated iron
- ◯ stone
- ◯ bricks

Agriculture

IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WERE GOOD HARVEST YIELDS? *

- ☐ 2007
- ☐ 2008
- ☐ 2009
- ☐ 2010
- ☐ 2011
- ☐ 2012
- ☐ 2013
- ☐ 2014
- ☐ 2015
- ☐ 2016
- ☐ 2017
- ☐ none
- ☐ don't know

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IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WERE BAD HARVEST YIELDS? *

- [ ] 2007
- [ ] 2008
- [ ] 2009
- [ ] 2010
- [ ] 2011
- [ ] 2012
- [ ] 2013
- [ ] 2014
- [ ] 2015
- [ ] 2016
- [ ] 2017
- [ ] none
- [ ] don't know

WHAT TYPE OF CROPS DO YOU CULTIVATE? (MORE THAN ONE POSSIBLE!) *

- [ ] Maize
- [ ] Mahangu
- [ ] Sorghum
- [ ] Vegetables
- [ ] Other

SPECIFY OTHER.

HOW MANY CATTLE DOES YOUR HOUSEHOLD OWN? *

DO YOU USE FERTILIZERS FOR CULTIVATION? *

- ◯ Yes
- ◯ No

Land_tenure

HOW MANY HECTARES OF FIELD DO YOU CULTIVATE *

one hectare = 100 x 100 meters = one large football field

AND THAT IS HOW MANY FIELDS? *

DID YOU LEAVE ANY FIELDS FALLOW IN THE LAST 5 YEARS? *

- ◯ Yes
- ◯ No

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DO YOU PLAN TO USE THEM AGAIN ONE DAY? *

- ◯ Yes
- ◯ No

ARE YOU CUTIVATING A FIELD THIS YEAR THAT YOU HAD LEFT FALLOW FOR SOME YEARS IN THE PAST BUT THEN RETURNED TO AGAIN?

- ◯ Yes
- ◯ No

FOR HOW LONG HAVE YOU BEEN USING THE LAND YOU CULTIVATE NOW? IN YEARS

DO YOU HAVE ANY PLANS TO CHANGE LANDS OR EXPAND YOUR CULTIVATION AREA IN THE NEXT FIVE YEARS? *

- ◯ Yes
- ◯ No

WOULD YOU SAY THERE IS SUFFICIENT LAND FOR EVERYONE? *

- ◯ Yes
- ◯ No

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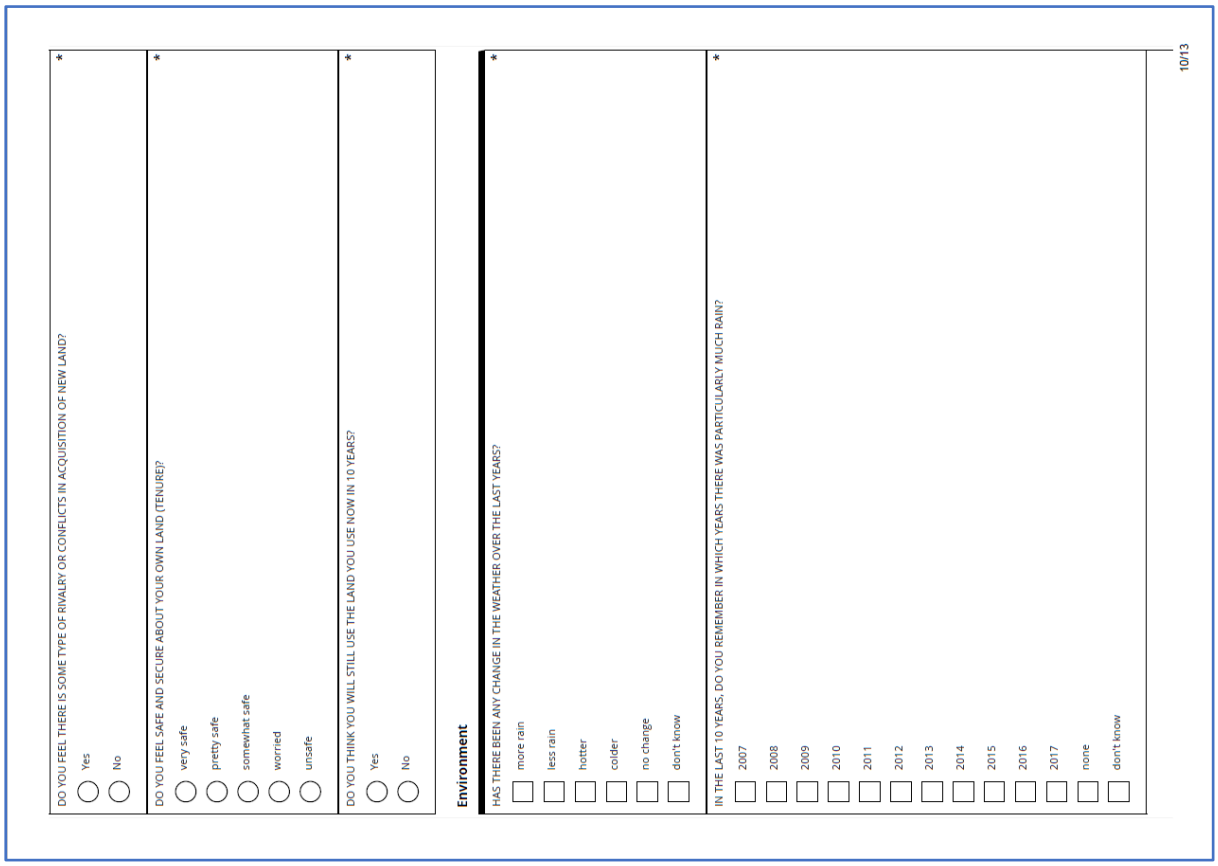

DO YOU FEEL THERE IS SOME TYPE OF RIVALRY OR CONFLICTS IN ACQUISITION OF NEW LAND? *
Yes
No

DO YOU FEEL SAFE AND SECURE ABOUT YOUR OWN LAND (TENURE)? *
very safe
pretty safe
somewhat safe
worried
unsafe

DO YOU THINK YOU WILL STILL USE THE LAND YOU USE NOW IN 10 YEARS? *
Yes
No

Environment

HAS THERE BEEN ANY CHANGE IN THE WEATHER OVER THE LAST YEARS? *
more rain
less rain
hotter
colder
no change
don't know

IN THE LAST 10 YEARS; DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY MUCH RAIN? *
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
none
don't know

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DO YOU REMEMBER IN WHICH YEARS YOU CLEARED FOREST FOR A NEW FIELD? *
none
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
never
don't know

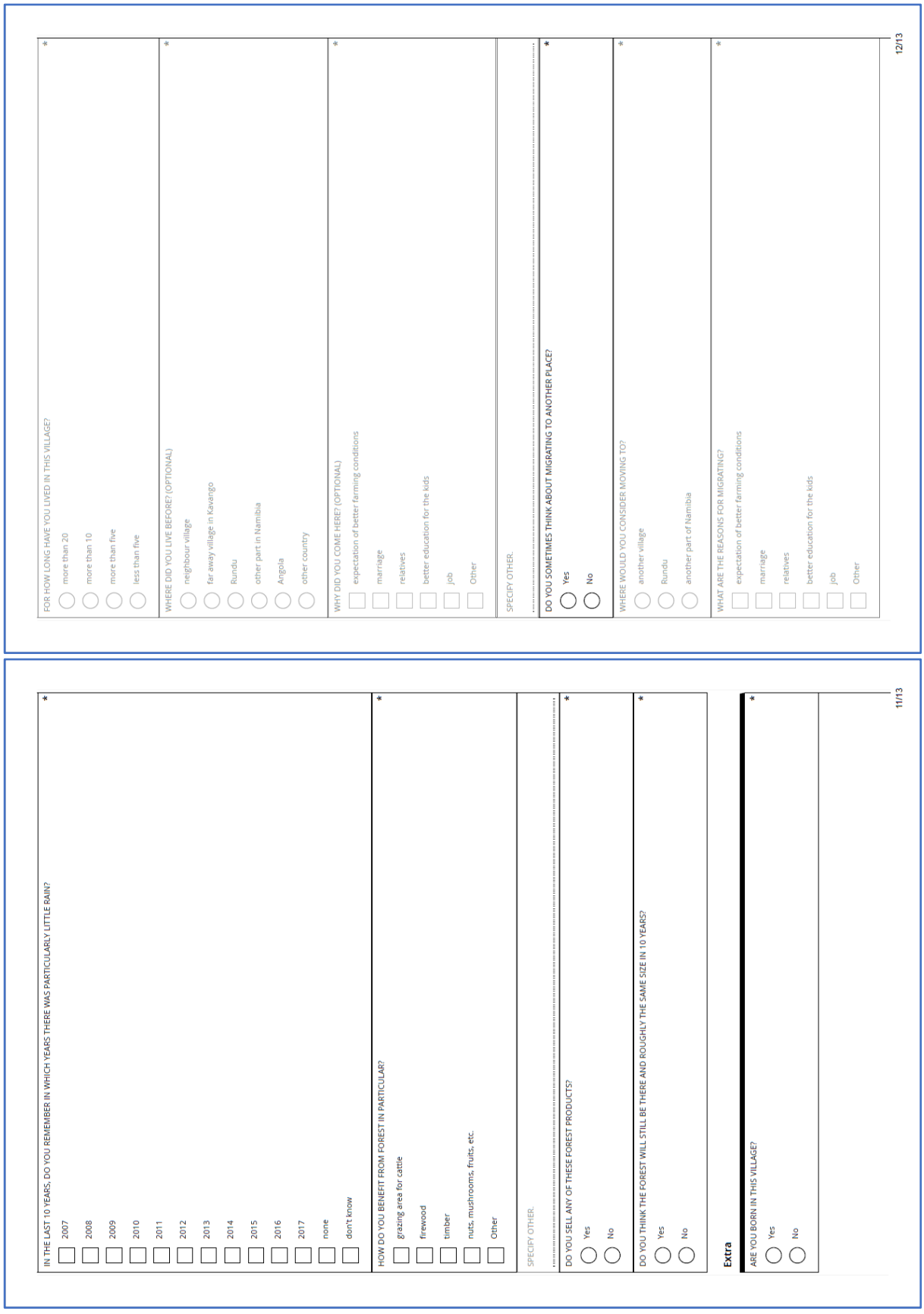
HOW MUCH ON AVERAGE WHEN YOU DID SO PER YEAR? *
(surface in hectare or meters, add unit!), put 0 if you they did not clear;
.....

DO YOU NEED PERMISSION FOR CLEARING? *
no
yes, from headman
Other

SPECIFY OTHER:
.....

HAVE YOU BEEN DENIED PERMISSION TO CLEAR IN THE LAST YEARS? *
Yes
No

9/13



FOR HOW LONG HAVE YOU LIVED IN THIS VILLAGE? *
- more than 20
- more than 10
- more than five
- less than five

WHERE DID YOU LIVE BEFORE? (OPTIONAL) *
- neighbour village
- far away village in Kavango
- Rundu
- other part in Namibia
- Angola
- other country

WHY DID YOU COME HERE? (OPTIONAL) *
- expectation of better farming conditions
- marriage
- relatives
- better education for the kids
- Job
- Other

SPECIFY OTHER.
.....

DO YOU SOMETIMES THINK ABOUT MIGRATING TO ANOTHER PLACE? *
- Yes
- No

WHERE WOULD YOU CONSIDER MOVING TO? *
- another village
- Rundu
- another part of Namibia

WHAT ARE THE REASONS FOR MIGRATING? *
- expectation of better farming conditions
- marriage
- relatives
- better education for the kids
- Job
- Other

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IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY LITTLE RAIN? *
- 2007
- 2008
- 2009
- 2010
- 2011
- 2012
- 2013
- 2014
- 2015
- 2016
- 2017
- none
- don't know

HOW DO YOU BENEFIT FROM FOREST IN PARTICULAR? *
- grazing area for cattle
- firewood
- timber
- nuts, mushrooms, fruits, etc.
- Other

SPECIFY OTHER.
.....

DO YOU SELL ANY OF THESE FOREST PRODUCTS? *
- Yes
- No

DO YOU THINK THE FOREST WILL STILL BE THERE AND ROUGHLY THE SAME SIZE IN 10 YEARS? *
- Yes
- No

Extra

ARE YOU BORN IN THIS VILLAGE? *
- Yes
- No

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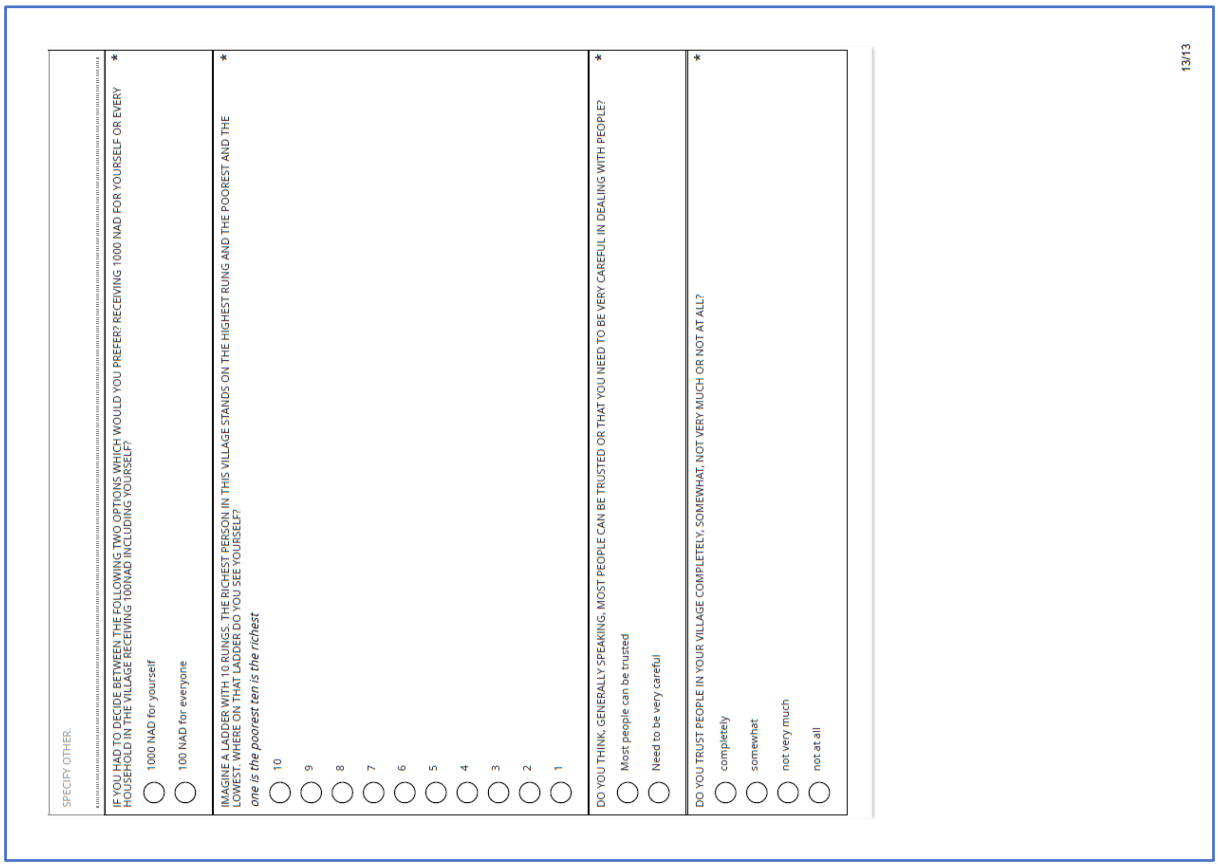

Supplement E: Information for data and analysis script request

Dataset and script for the data preparation and analysis ("do-file") can be made available upon request to the corresponding author. Game protocol and instruction are also available in the local languages spoken in Kavango, Namibia.

Chapter III:

On the Definition and Comparability of Individual and Group Incentives for Environmental Conservation

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3 The authors declare that there is no conflict of financial, general, or institutional competing interests

Incentive payments for conservation activities, also known as Payment for Ecosystem Services (PES), are increasingly being adopted worldwide (Salzman et al. 2018). However, if land is under joint ownership, payments conditional on individual performance are either too costly or impossible to implement. This is also the case if activities are difficult to monitor at the individual level and/or ecosystem services are only observed on an aggregate level (Engel 2016). In other cases, where both group and individual performance-based payments are feasible, the question arises as to which of the two types lead to better conservation outcomes. Table 1 illustrates four combinations of performance-based payments with respect to the conditionality and the recipient. Payments can be made either to a group or to individuals and can be conditional on the individual or the group performance. As three combinations include a collective dimension (categories A, B, C in Table 1)[1], we consider these as group payments. Gatiso et al. (2018) implement two group treatments (A and B) and compare them to fully individual payments (D) with the result that conservation effort is highest with D > B > A.1.[2]

Table 1: Individual vs. group payment characteristics
(grey shaded cells represent what can be considered group payments)[1]

		Payment Recipients	
		Group	*Individual*
Conditionality	*Group*	Type A	Type B
	Individual	Type C	Type D

Group conditionality (as in A and B) introduces two opposing mechanisms that potentially affect conservation outcomes:

1. Peer effects within the group that increase compliance with conservation.

2. Free-riding incentives since non-complying individuals can benefit from group payments while not bearing the cost for conservation. This represents a classical social dilemma situation.

[1] Own illustration.
[2] Type A of Table 1 is often implemented in practice due to lower transaction costs or equity concerns when donating money to schools or infrastructure projects in the village and can be easily compared to B. Gatiso et al. (2018) include three treatments: "community-based payments" (type A), "equity-based individual payment" (type B), and "performance-based individual payment" (type D). We think that collective payments conditional on individual performance (type C) offer few advantages and are of little interest.

In their response to Gatiso et al. (2018), Salk and Travers (2018) argue that the comparison by Gatiso et al. (2018) between type B and D is not entirely reasonable due to higher freeriding incentives in B. They state that any ceteris paribus comparison between group (B) and individual conditionality (D) should solely differ with regards to peer effects and not to peer effects *and* free-riding incentives. As pointed out by Salk and Travers (2018), peer effects are indisputably a strong driver of compliance in group settings. However, we argue that any group conditionality also inherently introduces freeriding incentives. Therefore, ceteris paribus comparisons as advocated by Salk and Travers (2018) provide little evidence for real-world PES schemes. Economic experiments are being increasingly applied as they are useful tools to single out specific aspects of PES (e.g., Andersson et al. 2018). Nevertheless, when looking at the positives of group conditionality (peer effects), we should not forget the negatives (free-riding incentives in groups). For policy recommendations, the relative importance of the two effects depends on the specific context at hand. Future research could look into the various aspects behavioral economists and others have identified to harness peer effects (e.g., Midler et al. 2015) and how these can be introduced in group-performance PES schemes.

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Chapter IV:

The Effects of Communication in Social Dilemma Situations:

Evidence from a Public Good Field Experiment

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JEL-classification: C71, C93, D83, H41, Q5

Funding and Acknowledgements

Funded by the Southern African Science Service Center for Climate Change and Adaptive Land-Use Management (SASSCAL) through the German Federal Ministry for Education and Research [grant number 01LG1201B]. Special thanks go to all participating communities for their hospitality and to all research assistants, in particular Adrian, Anastasia, Blondy, Cypriaan, James, and Moses.

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Highlights:

- Public good field experiment
- Framework disentangling communication effect on cooperation
- Comparing unrelated against coordinative communication
- Relevance of interpersonal relationships
- Norm to keep one's own promises more important than increased expectations after communicating about the social dilemma

Abstract:

Communication is well known to boost cooperation rates in social dilemma situations, but the exact mechanisms behind this have not yet been entirely understood. This paper studies the role of two forms of communication on public good provisioning in a field experiment conducted with farmers from small, rural communities in northern Namibia. In line with previous experimental findings, I observe a strong increase in cooperation when face-to-face communication is allowed before decision-making. I additionally introduce a condition where participants cannot discuss the dilemma but talk with their group members about an unrelated topic prior to learning about the public good game. It turns out that this condition already leads to significantly higher cooperation rates, albeit not as high as in the condition where discussions about the social dilemma are possible. This partly contrasts previously existent theories and findings. After ruling out better comprehension of the game and increased expectations of one's group members' contributions as the main drivers for the communication effect, I could instead identify the relevance of interpersonal relationships and the personal norm of keeping promises.

Keywords: communication, cooperation, field experiment, public good, norms

1. Introduction

One particular measure that has been observed to increase cooperation rates remarkably is allowing participants in social dilemma situations to talk to each other prior to making their decisions (Dawes 1980; Sally 1995; Balliet 2010; Ostrom 2010). Despite quite some research on the topic, exact mechanisms for this effect still remain mostly unclear (Lopez and Villamayor-Tomas 2017; Koessler et al. 2020). This study adds to solving the puzzle of identifying what determines decisions to cooperate and contributes to understanding how communication is able to raise cooperation in social dilemma situations.[1] Disentangling single elements may help harnessing the potential of communication in solving the dilemma: Does the simple act of talking reduce social distance, generate mutual trust and affect social preferences? Does talking about the social dilemma problem increase comprehension and create norms of cooperation? Or does it require mutual commitment to cooperate from all members in order to build positive expectations about the others' behavior? Based on previous theories and findings, I hypothesize that communication, in particular face-to-face communication, affects cooperation over various channels, some of which go beyond the mere content of the conversation. I develop a theoretical framework, shortly review relevant literature and then present results from a field experiment that compares three conditions: no communication as the baseline (Treatment 1), unrelated communication (Treatment 2) and coordinative communication (Treatment 3). The crucial distinction between the two communication treatments lies in whether or not group members already know about the upcoming public good game when they talk to their group members. This design therefore allows me to distinguish what I call the non-coordinative elements in communication from the coordinative ones. My study is conducted as a field experiment in northern Namibia with villagers from small, rural communities which brings about two advantages: Firstly, participants do already know each other, so that a communication effect cannot only be attributed to simply identifying and getting acquainted with one's group members. Secondly, it allows me to compare how communication interacts with different levels of previously existing social ties between participants, which has, to my knowledge, not yet been investigated before.

[1] Communication, in particular in experimental settings, usually refers to unrestricted face-to-face discussions between a group of participants that face such a social dilemma (Bicchieri and Lev-On 2007). Procedural standards in economic experiments further ensure anonymity of individual decisions which allows all individuals to reveal their true preferences without having to worry about retaliation by other participants during or after the experiment. Participants consequently only get to know their own and the group outcome but are unable to discover the individual decisions of the other group members (unless all group members unambiguously defect or cooperate, in which case the other's behavior can be deducted from the group outcome). Real money is offered in economic experiments to make preferences and decisions salient.

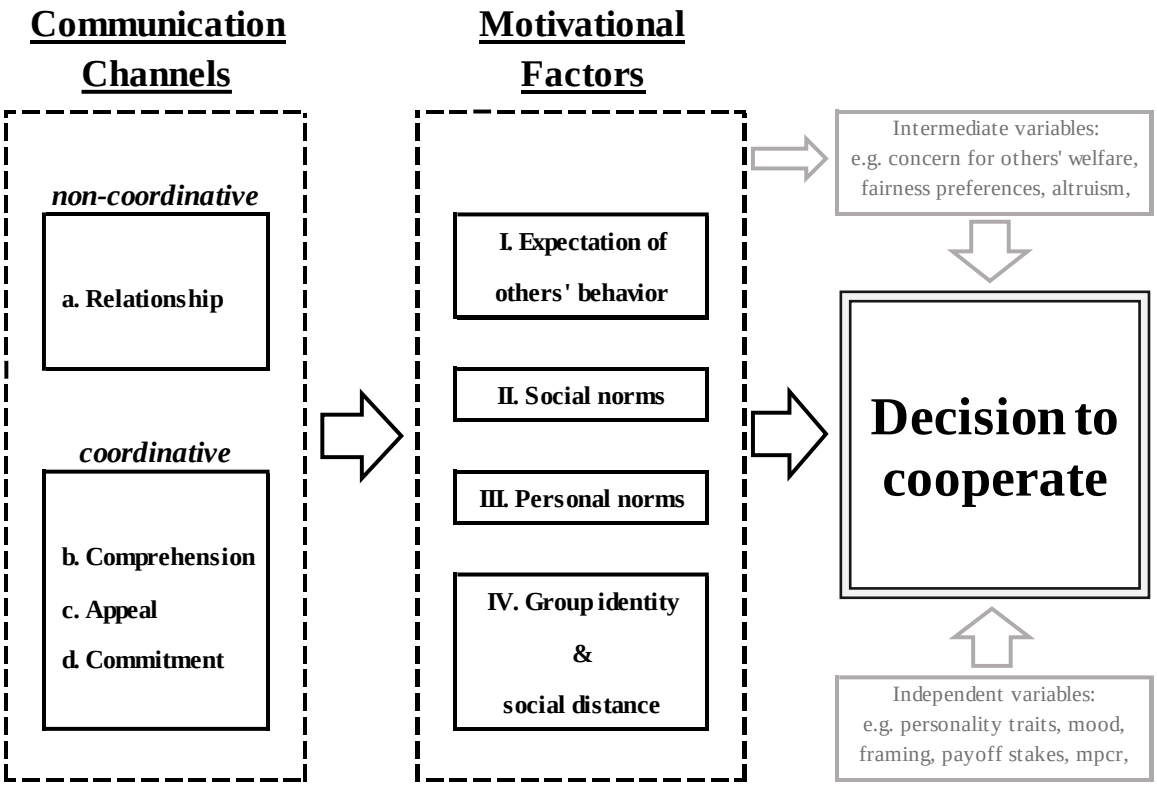
2. Theoretical framework

In their seminal researches on the topic, Dawes et al. (1977) identified three possible origins for the communication effect on cooperation: *1. getting acquainted, 2. discussions as exchange of relevant information and appeal to cooperate,* and *3. commitment to reassure one's own good intentions.* Endorsing its simplicity, I base my theory and experimental design on their concepts. As a substantial refinement, Dawes et al.'s second point is split up into "comprehension" and "appeal"; four potential communication channels consequently build the first major part of my framework (cf. Dawes et al. 1977; Kerr and Kaufman-Gilliland 1994).[2] These four channels I identify as relevant are depicted on the left-hand side of Figure 1[3] as (a.) relationship with one's group members, (b.) comprehension of the dilemma situation, (c). appeal to cooperate, and (d.) commitment.

However, communication can change outcomes in social dilemmas only by affecting individual motivations to cooperate. Beyond the four communication channels, I do, in the next step, also consider how these channels affect such motivations. Relevant elements here are expectations of others' behavior, social and personal norms as well as group identity and social distance**.** The framework is based on existing literature insofar as the elements included have been suggested as motivational drivers relevant for cooperation, but they have not yet been set in relation with each other. While the study attempts to decompose and structure single communication channels, they are, ex-ante, not meant to be distinctly linked to specific motivational factors. Next to motivational factors that affect cooperation, any decision about cooperation also depends on other, independent as well as certain intermediate variables, most of which will not be considered in detail. For a comprehensible build-up of the framework, I will, in the following paragraphs, firstly introduce and structure the motivational factors (i.e., the middle part of Figure 1) as they will later become relevant for discussing the suggested communication channels.

[2] Dawes et al. (1977) as well as Kerr and Kaufman-Gilliland (1994) mention that communication may improve comprehension of the dilemma situations.
[3] Own illustration.

Figure 1: Theoretical framework of decision making after communication[3]



I. Expectations of others' behavior

As the first motivational factor, I include expectations of how the other participants behave in the social dilemma. It seems intuitive that an individual's willingness to cooperate is higher if everybody else is also cooperating, even though it does not change the fact that defection still results in the higher payoff (i.e., the dominant strategy). In a situation where nobody else cooperates, on the other hand, said individual will likely not want to cooperate either. This is based on the concepts of conditional cooperation and reciprocity, which is widely acknowledged in economic literature on cooperation (Axelrod and Hamilton 1981; Fischbacher et al. 2001; Croson 2007; Kocher et al. 2008, Fischbacher and Gaechter 2010; Chaudhuri 2011). If an intervention, such as communication, is able to increase (mutual) expectations, then it will likely lead to higher cooperation outcomes (Kerr and Kaufman-Gilliland 1994). Expectations about others' behavior are also termed "beliefs" in economic literature (e.g., Fischbacher and Gaechter 2010).

II. Social norms

As the second factor, the framework considers the role of norms in cooperation. A social norm is understood as what is believed to be the appropriate conduct, or what someone believes that others expect them to do (Berkowitz 1972; Schwartz 1977, Cialdini et al. 1990; Cialdini et al. 1991; Brewer and Crano 1994; Kallgren et al. 2000, Fehr and Gaechter 2000). It is not formally composed or enforced, but can be socially enforced, also through sanctions (Brewer and Crano 1994; Fehr and Gaechter 2000; Bicchieri and Lev-On 2007). In short, it can also be considered an informal rule. Communication may introduce, activate or strengthen such norms. Since social norms are believed to be followed by the majority, this will also raise expectations of others' cooperative behavior. Next to directly affecting the decision to cooperate, they therefore also have an indirect, and presumably positive, impact through elevating beliefs, which triggers conditional cooperation. Breaking norms may result in the feeling of guilt, which can be interpreted as an intrinsic cost that individuals try to avert (Posner and Rasmusen 1999; Ostrom 2000; Charness and Dufwenberg 2006, Kessler and Leider 2012).

III. Personal norms

For the purpose of the presented framework, there is reason to distinguish between social and personal norms.[4] In contrast to social norms, which reflect someone's perception of how they are expected to behave or of what is considered as normal behavior, a personal norm describes what one believes the right thing to do according to their own, personal standards (Schwartz 1973; Schwartz 1977; Ajzen 1991; Cialdini et al. 1991). If either or both types of norms prescribe cooperation as the right conduct, individuals who abide to such norms will likely act accordingly.

[4] Cialdini et al. (1990) further distinguishes social norms into what is believed to be the normal (descriptive norms) and what is believed to be the appropriate behavior (injunctive norms). Beliefs about what is normal should consequently be reflected by expectations, whereas this is not necessarily the case for norms of appropriate behavior. In social dilemma situations, however, the concept of conditional cooperation renders it unlikely that social norms can be effective if they are not expected to be followed by others as well. In this sense, a norm that is believed to prescribe the appropriate behavior but is not followed by the majority can somewhat overlap with the concept of a personal norm. If an individual abides by a norm without believing in their peers doing so as well, then that individual adheres, according to my understanding of the concept, to a personal norm. In similar terms, Ajzen (1991), Smith and McSweeney (2007), Rivis et al. (2009), Schram and Charness (2015) as well as Mittelman and Rojas-Méndez (2018) distinguish between social and moral norms.

IV. Group identity and social distance

The fourth factor in my framework is built by group identity and social distance amongst participants. Group identity is in this context understood in a rather narrow sense as the perception of being part of a social group (Tajfel et al. 1971; Turner 1982; Spears 2011). It is somewhat similar to social distance, which describes the relationship between groups or individuals, how close they are, not spatially but in the degree of understanding and intimacy in their personal as well as social relationships to each other (Park 1924).[5] In theory, these two factors can be kept separate.[6] In practice, however, social distance and group identity are mostly interdependent and affect each other. For example, members that share a common group identity feel socially closer to each other and socially close individuals likely develop some form of group identity (cf. Park 1924; Driedger and Peters 1977).[7]

With decreases in social distance or elicitation of group identity, one's group members become more relatable so empathy and concern for their welfare may increase (cf. Schelling et al. 1968; Kerr and Kaufman-Gilliland 1994; cf. Bouas and Komorita 1996; Bohnet and Frey 1999). A number of studies have observed higher cooperation rates and willingness to help each other with socially closer individuals (Essock-Vitale and McGuire 1985; Yamagishi and Sato 1986; Thompson et al. 1998; Kollock 1998; Monsutti 2004; Peters et al. 2004; Bowles and Gintis 2004; Goette et al. 2006; Ruffle and Sosis 2006; Haan et al. 2006; Castro 2008; Boone et al. 2008; Apicella et al. 2012; Chuah et al. 2014). Similar preferences have also been found in increased trust and altruism (Glaseser et al. 2000; Buchan and Croson 2004; Rachlin and Jones 2008; Goeree et al. 2010; Cadsby et al. 2008; Etang et al. 2011; Binzel and Fehr 2013; Candelo et al. 2018). Such preferences can also be explained with evolutionary theories (Caporeal et al. 1989).

[5] I prefer this pristine definition of social distance, whereas other studies sometimes describe socials distance through actual manifestations, such as social and ethical affiliations, shared practices and customs or conformity in behavior (Akerlof 1997; Leeson 2008). Obviously, this overlaps with the concept of group identity. My study does, however, neither claim to define terms nor does it intend to empirically examine the differences between group identity and social distance.

[6] Group identity could, for instance, also evolve with non-identified group members, e.g., being member of a large group, in which case it might not directly affect social distance (cf. Driedger and Peters 1977; Kramer and Brewer 1986; Bicchieri 2002). Similarly, being friends with or being related to someone might affect decisions to cooperate without the formation of a specific group identity.

[7] Most literature on the communication effect therefore refers to only "group identity" and do not consider "social distance" as an additional factor.

According to the social identity theory and the self-categorization theory, a stronger identification with the group shifts the focus of attention away from the individual towards the collective target, which means that members of a group with a strong perceived group identity are more likely to seek maximizing the group benefit instead of their individual payoff (Turner 1975; Tajfel et al. 1979; Turner 1982; Kramer and Brewer 1984; Brewer and Kramer 1986; Dawes et al. 1990; Kerr and Kaufman-Gilliland 1994).

As depicted in Figure 1, the introduced motivational factors must not be understood as separate and independent, or even mutually exclusive. On the contrary, if communication decreases social distance or strengthens group identity, norms may become more salient and expectations of other's cooperative behavior might rise (Brewer and Kramer 1986; Roth 1995; Hoffman et al. 1996; Bohnet and Frey 1999; Bicchieri 2002).[8] Similarly, establishing or strengthening norms may also raise expectations, in particular, if one's own perceptions of appropriate conduct are being projected onto others.

Next, the four communication channels will be explained, which can be divided into "coordinative" and "non-coordinative" elements as illustrated in Figure 1. As "non-coordinative" I consider any conversation content that is not about the social dilemma, whereas "coordinative"[9] describes those channels that include discussions about the actual dilemma. For each channel, it will be considered how, in particular, they may manipulate aforementioned motivational factors and thereby, ultimately, affect the decision to cooperate.

a. Relationship (non-coordinative)

The first channel of interest is what I consider the "relationship" channel of communication. It encompasses any conversational content that affects each group member's relationship to each other, their social closeness and how they see themselves and the others in the group. This can entail non-coordinative topics like greeting each other, introducing oneself or others, and any type of small talk that is not about the social dilemma and the upcoming decision. However, also coordinative discussion topics, such

[8] So far, the discussion about the communication effect in cooperation has indeed focused mostly on either group identity or norms as the relevant explanation, with some authors favoring the latter (Kerr and Kaufman-Gilliland 1994; Bicchieri 2002).

[9] Instead of "coordinative" on could also use the terms "strategic", "relevant" or "dilemma-related" to describe this condition.

as explanations, appeals and commitments are not excluded from potentially affecting the group members' relationships to each other (cf. Schulz von Thun 1981).[10]

The relationship channel primarily decreases social distance and creates or strengthens the feeling of belonging to the group, which results in higher cooperation outcomes, as suggested by my framework (Kramer and Brewer 1984, Brewer and Kramer 1986; Dawes et al. 1988; Orbell et al. 1988; Kerr and Kaufman-Gilliland 1994). A direct and, even more so, an indirect impact on norms and expectations is (to some extend) also possible. Finally, the social effect of communication could, in theory, also have a negative effect on cooperation if participants only learn through communication that they do not like their group members or do not find them trustworthy.

b. Comprehension (coordinative)

As the second potential channel, communication can increase comprehension of the situation if some participants have not entirely understood the nature of the social dilemma. In experimental settings, discussions can help clarifying the rules and game mechanics (Dawes et al. 1977; Kerr and Kaufman-Gilliland 1994). It is, however, not clear what effect increased understanding could possibly have on cooperation outcomes. Conceivably, comprehending how mutual cooperation is in everyone's best interest could establish it as the preferable option, from a normative point of view. On the other hand, better comprehension of the social dilemma could also cause an individual to realize that defection always leads to a higher individual payoff and consequently to switch from an intuitive intention to cooperate to a deliberate decision to free-ride (cf. Kahnemann 2011).

[10] The communication channels presented in this study show a striking similarity to the "four-sides model" on general communication by Schulz von Thun (1981). In his model Schulz von Thun states that each message in a conversation includes not only the factual content, but also transmits information about the relationship between the participants, a self-revelation of the sender and an appeal to the receiver(s). To be precise, it needs to be clarified that in Schulz von Thun's original model, the three sides "self-revelation", "relationship" and "appeal" are all contained, mostly indirectly, in the actual words that are transmitted. In my model, on the other hand, "appeal" and "commitment" happen, primarily, on a factual level. Indeed, the experiment was partly motivated by Schulz von Thun's concept.

c. Appeal (coordinative)

The third relevant channel to consider is the (mutual) appeal to cooperate that likely arises in group discussions (Dawes et al. 1977). Such an appeal to cooperation is ideally supported by all group members and can thereby create or activate a social norm to cooperate in a very direct way (cf. Orbell et al. 1988). The direction of the effect on cooperation is generally expected to be positive. The important distinction to the first channel lies in whether individuals make their decision to cooperate out of their own deliberations or whether they are persuaded by the others to do so. Theoretically, however, there could also be a crowding-out if an individual feels pushed too harshly by the others' prompt for cooperation. The "appeal" channel also includes making threats of punishment. Even if such threats are not plausible, they could still have an effect on a socio-psychological and interpersonal level and that could go into either direction, from timid obedience to defiant defection.[11]

d. Commitment (coordinative)

The fourth and last potential channel is commitment, which has repeatedly been suggested as the most influential one in explaining the effect of communication on cooperation (Orbell et al. 1988; Ostrom et al. 1992; Kerr and Kaufman-Gilliland 1994; Bicchieri 2002). Commitment happens in group discussions if one or several members state their intentions to cooperate or even promise to do so.[12] Such statements are not necessarily binding and plausible as the actual, individual decisions can, depending on the setting, not be enforced or monitored.[13, 14] A cunning free-rider might even deliberately lie about their intention to cooperate in order to gain higher payoffs from

[11] In repeated games, threats can be plausible: Conditional cooperators may even announce during the discussion their willingness to cooperate in further rounds but only if the other group members also cooperate. Even from a free-rider's perspective, an early break-down of intragroup cooperation is not desirable and threats to cease cooperation are therefore plausible if decisions in the social dilemma have to be made several times with the same group.

[12] While the theory in this framework distinguishes between appeal and commitment, in practice, these two are likely strongly linked to each other. Announcing one's willingness to cooperate can be understood as an appeal to others to do so as well and an appeal for mutual cooperation can be interpreted by others as a statement of one's own intentions to do so.

[13] Since non-binding commitment is technically not really commitment, it is often referred to in relevant studies as "cheap talk" or "signaling" of intentions.

[14] The actual decisions as well as the final payments to each participant are generally kept anonymous in economic experiments. Free-riders do therefore not need to fear social sanctions or reputational effects. This might be different in real situations where monitoring and enforcement are possible and sometimes, economic experiments also allow (costly) punishment of deviators within the mechanics of the game. This as well as any other particular alterations in the cooperation scenario will then likely also play a role in the respective conversation. Similarly, if decisions are to be made over several rounds, reciprocal effects are possible and will likely affect decisions as well as the content of the conversations.

the others' cooperative efforts. Keeping promises and not lying to people are, however, considered strong and rather universal social norms. Indeed, Bicchieri (2002) argued that the communication effect in cooperation was based on the norm of promise keeping rather than a general norm to cooperate or increased group identity. Commitments made during group discussions might therefore evoke trust and rise expectations of high cooperation, even if they are not technically binding. Those, who condition their decisions on the expectation of their group members' behavior, will then cooperate more (Orbell et al. 1988). In other words, the effectiveness of non-binding commitment is based on the premise that a deliberate lie is a violation of norms far worse than simply not cooperating (cf. Orbell et. al. 1988). For participants in experiments, it might be difficult to distinguish the consequences of breaking norms in the experimental setting from doing so in reality. Lying, in particular, was found to be psychologically similar in experiments as in the real world and is therefore easily associated with the danger of negative consequences and retaliation (Dawes et al. 1988). It was pointed out that keeping promises could be both a social and a personal norm (Kerr et al. 1997; Bicchieri and Lev-On 2007).

To sum up, there may well be a double effect of commitment: Firstly, individuals who made a commitment during the group discussion might feel bound to fulfill their own promise due to social and personal norms. Secondly, based on mutual trust in such commitments, expectations about the other group members' intentions to cooperate increase, which reciprocally further boosts cooperation.

3. Discussion of empirical literature and development of hypotheses

Several previous experimental studies have tried to disentangle the non-coordinative elements of the communication effect from the coordinative ones. Most of them follow an approach that is complementary to ours as they minimize any unrelated communication while only leaving the option to coordinate by sending written messages, partly anonymously, for example on paper or in chats through computer terminals. Summing up findings, it turns out that, while written communication also significantly increases communication, it is not quite as effective as face-to-face communication (Frohlich and Oppenheimer 1998; Bochet et al. 2006; Bicchieri and Lev-On 2007; Balliet 2010). In his meta study, Balliet (2010) observes the same across a large number of studies and points out the relevance of this finding: The mere content of conversations can easily be exchanged by modern communication means like emails and

telephone, yet on many occasions in business, politics and science, meeting in person remains important, even though it involves higher costs and consumes more time for travelling in order to meet each other.[15] Jensen et al. (2000) and Broosig et al. (2003) test even finer nuances by comparing various communication modalities like written messages, phone and video calls as well as face-to-face communication. It is found that the broader or "richer" a communication medium is, the better it is able to increase cooperation outcomes (Bicchieri and Lev-On 2007). The missing pieces in written communication in comparison to face-to-face communication are commonly explained by body language, facial expressions, eye gaze, the tone of voice and possibly other, more subtle cues (Roth 1995; Kurzban 2001; Bicchieri and Lev-On 2007).[16]

On the other hand, it must be considered that, by allowing only written messages, the non-coordinative channel is not necessarily entirely eliminated. Visual and tonal cues might not be available, but phrasing style and choice of words are still able to transmit information that go beyond the pure content and can further affect the relationship between the conversation partners. Wilson and Sell (1997) tested communication in a public good experiment over a computer terminal, where players could say nothing verbally but only signal their intended contributions as numbers. By doing so communication was truly reduced to the (non-binding) commitment channel. Interestingly, they did not find an increase in contributions; on the contrary, (forced) signaling of one's intentions resulted in lower contributions than a baseline without any communication[17]. Similarly, Chen and Komorita (1994) as well as Bochet et al. (2006) conducted experiments that allowed participants to state their intended contributions to a public good, but there were no positive effects on cooperation compared to no-communication conditions, either. Also, Dawes et al. (1977) already added public signaling in addition to unrestricted communication about the dilemma but it did not raise contributions above the level of unrestricted communication without signaling. Results from these studies indicate that non-binding commitments or stated intentions to cooperate alone are insufficient in explaining the communication effect.

In order to find out about the role of interpersonal relationships affecting cooperation, attempts have been made to test the effect of non-coordinative communication only, without the

[15] One could even interpret communication as a second order dilemma: everyone wants to enjoy benefits of higher efficiency after communication, but nobody likes to bear the cost of establishing communication.
[16] Kurzban (2001) tested some of these cues individually and found some evidence for increased cooperation after group members exchange mutual eye gaze or light physical contact, but this observation only seems valid in male sample groups.
[17] In the literature this type of non-binding signaling is often termed as "cheap talk". I do, however, avoid the term as the reader might easily confuse "cheap talk" with irrelevant "smalltalk", which means quite the opposite in this context.

possibility of coordination. A promising way to do so is to allow only unrelated (i.e., non-coordinative) communication between participants.[18] This can be understood as the counterpart to the studies previously discussed, as the idea is not to eliminate the non-coordinative channel but to remove the coordinative elements; testing whether unrelated discussions without coordination already affect decisions to cooperate. Dawes et al. (1977) were the first to test this by asking groups of participants to estimate the population proportions of different income levels of a particular US state as a communication task. Similarly, Bouas and Komorita (1996) hypothesized that finding consensus on any topic that was relevant to the participants could evoke group identity in group discussions. Both studies, however, found no effect of unrelated discussions. Higher cooperation rates in comparison to no talking were only achieved by groups that could actually discuss the dilemma. On the other hand, Kurzban (2001) found an increase in cooperation after allowing unrelated, non-coordinative communication via computer messages, which indicates that there can still be some effect even if the communication does not happen face-to-face, but leaves us with an inconclusive overall picture on the role of non-coordinative communication.[19] Looking at trust games, however, there is empirical evidence supporting a positive effect of unrelated communication (Buchan et al. 2006)

In conclusion, coordinative communication alone, without free, unrestricted discussion, is not found to be effective in increasing cooperation, and evidence for the effect for non-coordinative, social communication without the possibility to coordinate is, at best, mixed. Possibly, coordination is only effective, if both the coordinative and the relationship channels are activated. In particular, appeals to cooperate and commitments made during discussions might only be salient in groups that have built a certain amount of social closeness and trust to each other (Hardin 2003; Simpson 2007; Barbalet 2009).

[18] This is sometimes termed as irrelevant communication. If the communication was truly irrelevant, however, one would not need to bother testing its effect. What is meant by "irrelevant" in this context is that the communication is not about the social dilemma, but about some other topic. Instead, I use "unrelated" or "non-coordinative" communication as preferable terms.

[19] Kurzban (2001) admit that there were some methodical problems with the implementation of the unrelated communication treatment. Participants were allowed to write in a chat for 30 seconds just before decision making and they were told not to discuss dilemma or make pledges in this chat. Some participants did, however, not obey this instruction.

For my experiment, I extend and improve the previously applied approaches by asking the participating groups to discuss a given but unrelated topic with their group members before instructing them about the public good game. The particular advantage is that, even though a discussion topic was given in order to homogenize conversations, the content was not externally restricted and, possibly more importantly, not perceived by participants as restricted in any way. Previous studies have prohibited coordinative talk as participants were already aware of the upcoming game (cf. Bouas and Komorita 1996; Kurzban 2001). In my view, this could have detrimental and incalculable behavioral side effects. In the field setting of this study, social relations are partly pre-defined, as participants do already know each other. If communication effects on cooperation are found in this setting, then it can be concluded that they derive from the actual interaction in the conversation and not just from mere identification and getting-to-know each other.[20] Also, pre-existing social ties between participants measured as the number of family members and friends in their group are considered as one scalable manifestation of social distance, which is considered as a central element in my framework. Lab-experiments with student samples do not usually take into account existing social relationships between participants. Effects of communication can be expected to be different in field settings, where participants come from small communities, knowing each other rather well. Indeed, previous evidence on the communication effect from field experiments is more heterogeneous than results from the lab. While positive effects of communication on cooperation are regularly observed (e.g., Cardenas et al. 2004; Cardenas and Ostrom 2004; Velez et al. 2010), this was not always the case (Velez et al. 2012). Ghate et al. (2013) found that communication was not necessary to increase cooperation if participants already show a high level of trust. Having a closer look at the interaction between social ties and communication might therefore be worthwhile and could help finding out from which factors in particular communication effects originate. In-group favoritism is a rather well-established finding in economic psychology (Tajfel et al. 1979; Akerlof 1997; Buchan et al. 2006). To my knowledge, this has not yet been considered in studies on the effect of communication on cooperation.

[20] Dawes et al. (1977) consider "getting acquainted" as one possible explanation for the communication effect. This condition is sometimes understood as identification of fellow group members (Kerr and Kaufman-Gilliland 1994). However, in all treatment conditions of this study, group members are able to see and identify each. If identities were kept anonymous in the no-communication treatment, then the effect of communication could not be distinguished from effects of identification, which is, to my understanding, not supposed to be part of studying communication. The distinction between identification and anonymity was actually also examined by us as part of the same research project and results will be reported in another paper (Hoenow and Pourviseh, in preparation).

With the experiment presented in this paper three hypotheses will be tested: Firstly, it is compared if non-coordinative communication is able to increase cooperation compared to a no-communication baseline group as established in hypothesis 1:

H1: Non-coordinative communication in groups has an effect on cooperation

Secondly, I compare between two distinct forms of communication, namely coordinative and non-coordinative discussions. The second hypothesis is therefore formulated as:

H2: The effect of non-coordinative communication is different from coordinative communication

And thirdly, I test for interactions between both communication conditions and pre-existing social relations as given in the village-community setting of the research sites.

H3: Communication effects depend on or interact with previously existent social relations in groups

4. Method

4.1 Research setting and participants

The experiments were conducted from April to June 2017 in 12 randomly selected rural villages in Kapako district (Kavango West) and Ndiyona district (Kavango East).[21,22] For the selection, villages that had formerly been visited for similar research projects were left out. Further preconditions were that there were more than 80 inhabitants and the village was not more than a day's drive away from the nearest tar road[23]. The original total sample size was 216 participants, 72 in each of the three treatments. After data cleaning and dropping observations from participants that failed to answer the control questions correctly, I ended up with 172 valid observations. All tables, figures and results reported are based on the cleaned dataset if not otherwise indicated. Table 1 summarizes the socioeconomic characteristics of the sample.[24]

[21] The experiment was embedded in a bigger study about cooperation, deforestation und development in rural Kavango, which, in turn, constituted a part of the SASSCAL research project (see funding and acknowledgements).
[22] The names and the positions for all villages are shown in the supplementary materials (A.1).
[23] A sufficiently large number of inhabitants was necessary in order to allow a comparable and random sampling. Distance from the tar road had to be limited as particularly villages that were hidden deep inside the forests were not only difficult to find but also usually rather small and sometimes deserted. For logistical reasons and due to limitations in time and resources, I could therefore not make the endeavor to include them in the sample to draw from.
[24] A table for socioeconomic characteristics split by treatments conditions can be found in the supplementary materials (A.2). There, I also test for equality of subsamples and it turns out that most, but not all, variables are equally distributed. It is therefore reasonable to add these as control variables in the regression models.

Table 1: Summary statistics and variable description

Variable	Obs	Mean	Std.Dev.	Min	Max	Variable Info
Control_wrong*	216	0.20	binary	0	1	Control question wrong answer
Expectation	172	5.91	2.72	0	10	Belief of others' contribution
Trust in group	171	2.50	0.83	0	3	Trust in group members (0-3)
Family	172	1.19	1.18	0	3	Relatives in group
Friends	172	0.69	0.98	0	3	Friends in group
Age	172	35.99	14.49	18	84	Age of participants
Female	172	0.60	binary	0	1	Gender (1 for female)
Schooling_years	172	7.15	3.64	0	14	Years of schooling
Head_of_hh	172	0.41	binary	0	1	Head of household
Migrant_10	172	0.18	binary	0	1	Moved to village (<10years ago)
Socialladder	172	2.72	2.37	1	10	Self-assessed social status
Cattle_owned	172	9.16	17.16	0	120	Number of cattle owned

*variable "control_wrong" shows all 216 observations, i.e., includes uncleaned data
Source: own calculations based on collected data

The majority of the rural population in Kavango is engaged in agriculture with crop farming as the primary component of their livelihood and cattle farming taking the second relevant role (Namibian Ministry of Lands and Resettlements 2015). Farming is often on subsistence level and only partly integrated into markets. The Kavango region is further characterized by a young and growing population, most of which enjoyed some years of formal school education. The addressed particular advantage of choosing Kavango villagers as participants for this research are pre-existing social ties between them. Villages are small in population size[25] and villagers do usually live in the same place for many years or even a lifetime, which means that the majority of the participants could be expected to have known each other before the experimental workshops. In addition, there are village meetings as well as social and religious gatherings held regularly and some households work together in certain agricultural tasks. Kinship relations can also be found between many households.

In preparation of the experimental workshops, each village's headperson was visited several days ahead in order to arrange an appointment for a village meeting so that all villagers could be informed and invited in time. It was made clear beforehand that some monetary compensation would be offered for participating but also that only a certain number of participants would be able to take part in the research workshops. At the beginning of each village meeting, 24 participants were randomly drawn by lot amongst those who expressed willingness to participate. This selection procedure was considered fair by almost everybody. The same lots also determined the allocation to one of two treatment groups per village. These treatment groups of 12 players each were then spatially separated and we explained to them the procedure of the workshop as well as the instructions of the public good game according to the

[25] On average 642 inhabitants.

respective treatment condition.[26,27] Each treatment group was supervised by one experimenter and one local research assistant for interpretation. The allocation of both was ex-ante randomized, so that all treatment conditions were played by all combinations of experimenters and assistants. For the public good game the 12 persons per treatment were later again split into three groups of 4 players.

The rules of the game were made very clear with the help of posters and giving examples for outcomes, but without valuing or recommending any particular behavior for the game (Figure 2)[28]. Special attention was paid to making clear that the game was not a "zero sum" situation about dividing the money, but that cooperating actually increased the total benefits for the group as a whole. Tests for comprehension were carried out with the group and individually in private before the game started.[29] While individual control questions were asked, research assistants also asked about the player's belief about their other group members' average contribution. Correctly stated beliefs were incentivized with an additional 20N$ reward in the final payments.[30] The assistants also gave additional help and instructions to those who did not understand all instructions right away. We did however make sure that everybody was as fit as possible for the decision making in the real game and did not require assistance once the game started. Hence, all game decisions could be made by the players individually and anonymously. For the decision making in the public good game, plastic coins were used as game currency. These were to be put into two differently colored envelopes, one of which represented the individual and the other one the group account. Players sequentially moved with both envelopes to a separate location to make their contribution decision in private. The envelopes that contained the players' decisions were then put into a basket, so that contributions were kept anonymous and could afterwards only be attributed to the players' ID numbers, guaranteeing some degree of anonymity in decision making not only towards the group members but also towards the researchers. Participants' names were never asked and can therefore not be linked

[26] Protocols and instructions were translated by the local assistants from English into the respective local languages and then translated back into English by another assistant in order to ensure that all translated instructions were on point. Also, all wordings and phrases used in the instructions were discussed intensively with the local assistants in preparation of the experiment as to make all instructions clear and easily understandable.
[27] Experiment protocols and game instructions can be found in in the supplementary materials (B).
[28] Own illustration.
[29] If one or both of the control questions were answered wrongly, the player would still participate in the experiment and receive their payment but the respective observation would not be considered in the analysis. This led to slightly unequal sample sizes for the two treatments.
[30] Stated beliefs were considered correct if the actual average (not including the player's own contribution) was in a range of ± 1 coin of the estimation. For example, if a player guessed 7 coins and the average contribution of the other three group members was 6.33 coins, the 20N$ bonus was granted. Getting correct information about expectations is important in order to gain insights about reasons behind potential effects. The additional incentives mean to make participants put some effort into guessing the correct number and not just give a short, thoughtless statement.

to their ID-numbers in the game. A research assistant stayed with the remaining group members to make sure they did not talk or communicate in any way while waiting for their turn. After making their decisions, players proceeded to go to the snack area for a break and were then interviewed individually for the survey. Survey questions can be found in the supplementary materials (C.) and include, amongst others, questions about socioeconomics, the relationship with their group members and trust. Following Bogardus (1925), pre-existent social relationships were measured in three categories, as "family", "friend" and "other"[31]. Trust in one's group members was collected as an ordinal variable on a scale from 0 ("not at all") to 3 ("trust completely"). Payments according the participants' and their group members' decisions were done in the very end individually and in private. The whole workshop took about 4 hours in each village. Payoffs averaged at 97N$ (= 7.32US$) per participant, which is more than an average local wage for a day's work. The theoretical range was between 25 and 145N$ (≈2 and 11 US$), including the bonus payment for correctly stated expectations.[32]

4.2 The public good game

For the experiment, an unframed, single-round, standard public good game was chosen. Participants could earn real money according to their own and their group members' decisions. There were always 4 players in a group playing the game together. Each player received a private endowment of 10 coins and had to decide about how much to keep and how much to contribute to a group account. The game was framed neutrally with coins, private and group accounts, so that associations towards any particular applications were avoided.[33] It was possible to contribute any number of coins between 0 and 10. After all players had made their decision, contributions to the group account got doubled and then distributed equally to all four players regardless of how much each player contributed individually. The socially optimal

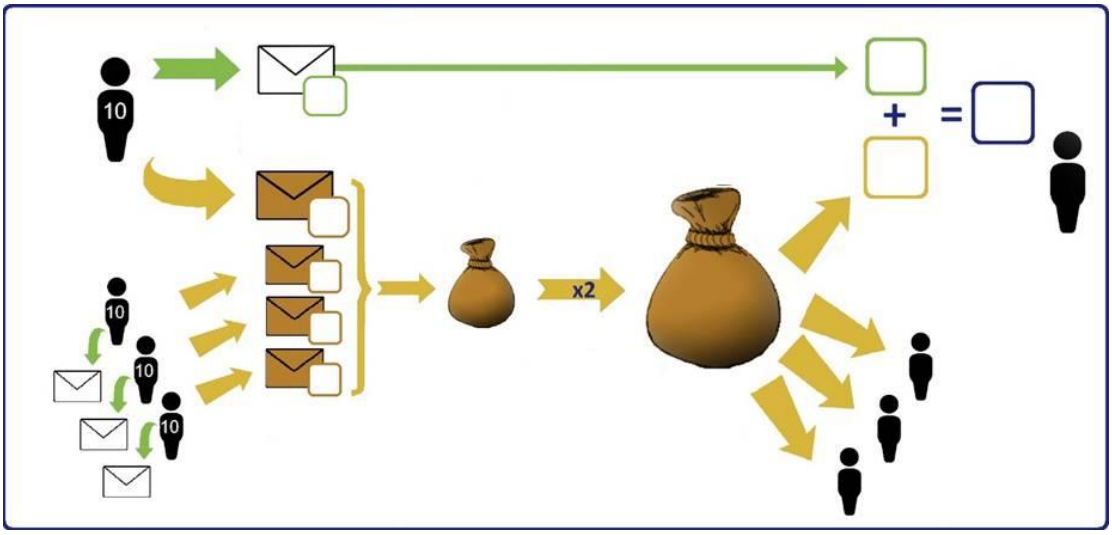
[31] Originally, there were two more subcategories "acquaintances" and "stranger/unknown" but it turned out that there were so few strangers mentioned that I decided to group these two categories together into one. Fewer categories also reduce problems with multicollinearity of explanatory variables in the regression model.

[32] Payoffs were set after pre-testing for calibration and allowing a reasonable final compensation for participating. A game currency was used in order to keep the number of coins used in the game low and with a range of possible contributions between 0 and 10 it can easily be compared to similar studies. On the downside, is required an additional step to calculate how much one unit of game currency was in real money. The conversation rate was one to five (1coin = 5 N$ and 1 US$ = 13N$).

[33] It happened twice that some participants asked the experimenters about what the game is supposed to represent and started to discuss possible applications during the game instructions. These discussed applications included collecting money for building a new well or a school for the community, which, in my view, equals a framing. We made the decision to drop all observations from these two villages right there in the field and re-sampled two randomly selected villages as substitutes. If, on the other hand, similar discussions and suggested applications occurred in the group discussion in treatment 3, we could firstly not find out as conversations were private and secondly, it would be considered a legitimate element of such group discussions.

outcome was reached when everyone decided to contribute all of their endowment, i.e., 10 coins. Individually, however, one could always reach a higher pay off by not contributing at all (Nash equilibrium). Since the public good game was one-shot and anonymous, no reciprocity effects over rounds were possible and contributions supposedly measured the participants' pristine preferences (Rand and Nowak 2013).[34]

Figure 2: Poster used to explain the public good game[28]



The public good game payoffs can be formalized with the following equation for individuals **U_i** (equation 1) and for the group as a whole **S** (equation 2):

Equation 1:

$$U_i(e_i, c_i, c_j) = e_i - c_i + \frac{m}{n}c_i + \frac{m}{n}\sum_{j=1}^{n-1} c_j$$

Equation 2:

$$S(e_i, c_i) = \sum_{i=1}^{n} U_i(e_i, c_i) = n * U_i(e_i, c_i) = n * e_i - n * c_i + m\sum_{i=1}^{n} c_i$$

U_i = Utility of player i
e_i = endowment player i
c_i = contribution decision of player i, $\in\{0, \ldots 10\}$
c_j = contribution decision of player j, $\in\{0, \ldots 10\}$
m = multiplier of group account
n = number of players in the group
S = Sum of all players' utilities

[34] In a repeated game, on the other hand, it might be in one's own interest to cooperate in the beginning in order to keep cooperation rates up and benefit from sustained cooperation.

With an endowment (e) of 10 coins, a group size (n) of 4 player and the social multiplier (m) set to 2, the payoff equation becomes (equation 3):

Equation 3:

$$\boldsymbol{U_i(c_i, c_j) = 10 - c_i + \frac{2}{4}c_i + \frac{2}{4}\sum_{j=1}^{3} c_j}$$

$$\boldsymbol{U_i(c_i, c_j) = 10 - \frac{1}{2}c_i + \frac{1}{2}\sum_{j=1}^{3} c_j}$$

With c_i being the only variable that player *i* can manipulate, it becomes obvious that contributing nothing is the individually best option. For the group payoff (equation 4), there is:

Equation 4:

$$\boldsymbol{S(c_i) = 4 * U_i(c_i) = 40 - 4 * c_i + 2\sum_{i=1}^{4} c_i}$$

$$\boldsymbol{S(c_i) = 40 - 4 * c_i + 2 * 4 * c_i}$$

$$\boldsymbol{S(c_i) = 40 + 4c_i}$$

This shows that, for the group, contributing as much as possible leads to the highest utility. Since c_i is capped at 10, this is the social optimum in the game. The minimum payoff in the experiment is 5 coins (25N$) for someone, who contributes everything while in a group with three free-riders. The maximum payoff is 25 coins (125N$) for a free-rider in a group with three cooperators.[35]

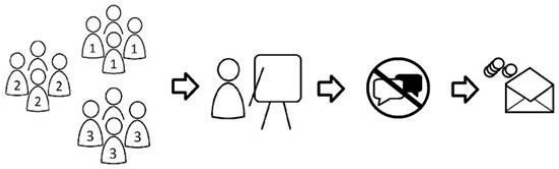
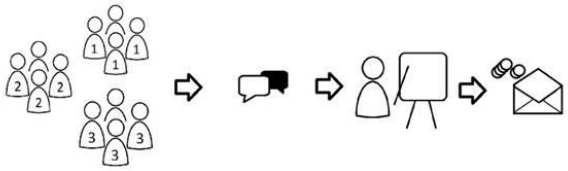
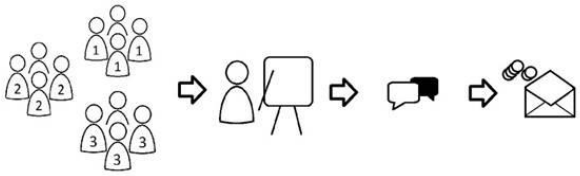
The following figure (3)[36] summarizes the three treatment conditions. In order to keep everything except the communication conditions comparable, participants in the no-communication group could identify their group members and were given a few moments of time before making their decisions to substitute for potential deliberations about the explained social dilemma situation. While the second treatment only allows unrelated discussions, the third treatment can be considered as what is usually understood by communication. Discussions were never listened to or even recorded, in fact, for the second and the third treatment condition experimenters and research assistants deliberately moved out of hearing distance from the groups so that they could talk freely.[37] Groups were spatially divided for discussions so that other groups could not be listened to and influence the content of discussions or the outcome.

[35] Plus, the bonus for correctly estimating the other players' contribution, the maximum amount that can be earned is 125 + 20 = 145N$.

[36] Illustrations used within Figure 3 are my own.

[37] As a measure of compliance to the intended treatment conditions, we ex post asked participants in the survey about the content of their discussions. While this elicitation method might not be specifically precise, it still turned out that 87% in the second treatment complied with their task of discussing agriculture and 55% of participants in

Figure 3: Graphical illustration of treatment conditions[36]

Treatment and Description
Treatment 1: No Communication In the baseline treatment participants play the public good game with revealed identities of their group members. The participants are allocated in groups of four according to the numbers on their ID-cards. They are, however, not allowed to communicate with each other. Before decision making starts, an explicit statement is made by a research assistant that the groups are playing the game together as allocated. 
Treatment 2: Non-coordinative (unrelated) Communication In the non-coordinative communication treatment participants are asked to discuss a given but unrelated topic for five minutes in groups before learning about the public good game. They are allocated in groups of four according to the numbers on their ID cards and given the task to discuss how different rainfall and changes in climate affects agricultural outputs and how adaptation measures could be taken.[38] No communication is allowed after learning about the game rules. 
Treatment 3: Coordinative Communication In the coordination treatment participants learn about the rules of the public good game first and are then allowed to talk to their group members for five minutes before making their decisions. Hence, players have the opportunity to discuss the social dilemma and coordinate their actions.[39] Decisions are still made in private. 

the third treatment answered that they coordinated decisions with their group members, even though in this treatment no particular discussion topic was suggested externally. For privacy reasons, we did not ask more detailed questions about the content of discussion, such as whether agreements or promises were made.

[38]This topic was chosen because it is not a controversial one but something that is related the participant's everyday life. Each participant should be able to understand the subject and be able to contribute something to the group discussion if they like to. At the same time, it is sufficiently relevant as to not make participants wonder about the topic's purpose or get excessively bored discussing it. In pre-testing of the experiment, participants were initially allowed in this treatment to talk about anything they like, but it happened several times that participants felt insecure as they did not know what to talk about with their group members so no real discussion took place. It was therefore decided to externally specify the topic as a basis for discussion.

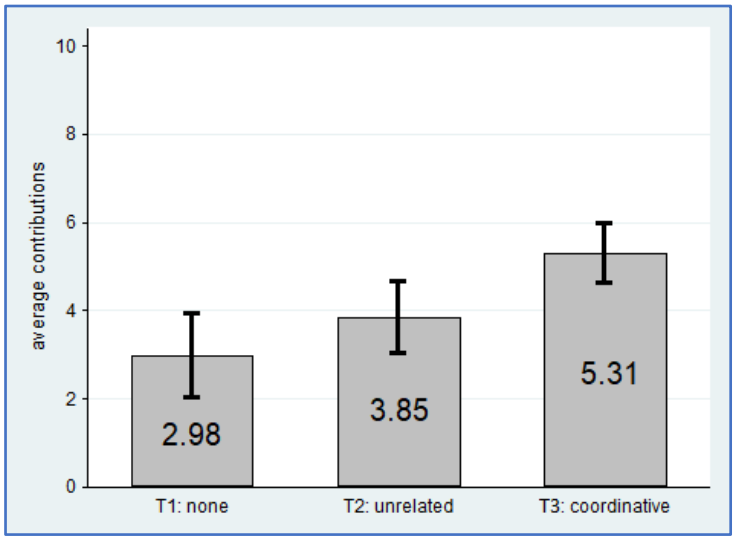
[39] Participants in this treatment were not specifically encouraged to discuss the dilemma or to coordinate, but were just told that they were allowed to talk to each other about anything.

4.3 Course of analysis

The main results about contributions to the public good game are firstly presented graphically (figure 4)[40]. Next, average contributions are tested for significant differences by Mann-Whitney U-tests (table 2) and finally, regression analyses for each single treatment and for the whole sample are applied (table 3). In the regression analyses I also investigate effects of pre-existent social ties and how they interact with each communication treatment. After showcasing the contributions to the public good games, I also look into trust and beliefs about other group members' contributions in order to obtain further insights about the reasons for the communication effect (figure 5)[41]. While beliefs were stated as incentivized estimates of the others' average contributions, trust was added as a survey question on how much a participant trusts their group members. In a next step, I analyze comprehension by checking whether communication affects the share of participants who correctly understood the game mechanics, i.e., answered both control questions correctly (table 2). In this context it is also investigated if and how comprehension affects contributions to the public good. All regression tables show ordinary-least-squares estimations with standard errors clustered on group-level[42] and include a set of control variables as presented in table 1. Tests for multicollinearity, heteroscedasticity and normal distribution of residuals as well the distribution of the dependent variable can be found in the supplementary materials (A.3).

5. Results

Figure 4: Average contributions by treatment group[40]



[40] Own illustration based on collected data, created with Stata 15 statistical software.
[41] Own illustration based on collected data, created with Stata 15 statistical software.
[42] Group-level of four players playing the public good game (and not treatment sampling group-level).

Table 2: Comparisons between treatments and tests

Treatment No:	T1	T2	T3	T1-T2	T1-T3	T2-T3
Communication:	**none**	**unrelated**	**coordinative**			
	mean (sd)	mean (sd)	mean (sd)	**difference** (p)	**difference** (p)	**difference** (p)
Contribution	2.98 (3.52)	3.85 (2.60)	5.31 (2.91)	0.87** (0.042)	2.32*** (0.000)	1.45*** (0.004)
Expectation	5.90 (2.71)	5.60 (2.87)	6.18 (2,62)	-0.30 (0.573)	0.28 (0.672)	0.58 (0.317)
Trust in group	2.46 (0.84)	2.36 (0.88)	2.56 (0.80)	-0.10 (0.563)	0.11 (0.441)	0.21 (0.185)
Mean of within-Group std. dev.	2.451 (1.496)	2.588 (1.435)	1.543 (1.206)	0.14 (0.782)[A]	-0.90* (0.053)[A]	-1.04** (0.024)[A]
Observations (Share of 72)	52 (0.72)	55 (0.76)	65 (0.90)	3 (0.567)	13*** (0.006)	10** (0.025)
Contribution if misunderstood	4.15 (3.99)	5.06 (4.19)	6.86 (3.58)	1.17[B] (0.323)	1.20[B] (0.356)	1.55[B] (0.165)
Share correctly Stated expectations	29%	36%	55%	8% (0.404)	27%*** (0.004)	19%** (0.037)

- A: T-test for mean of within-group std.dev. (on group level, therefore 18 observations per treatment)
- B: based on the answers given to both control questions; comparisons not across but within each treatment group
- P-values in parentheses for test results according to Mann-Whitney U-tests
- Chi-squared test for control question comparison and correctness of estimated expectations
- Standard deviations in parentheses for mean values
- Significance levels: * p<0.10, ** p<0.05, *** p<0.01

Source: own calculations based on collected data

5.1 Contributions to the public good

Comparing average contributions across the three treatments reveals a clear levelled effect of communication: non-coordinative communication as in treatment 2 raises cooperation significantly, but not as much as coordinative communication as in treatment 3 (table 2 and figure 4). Both increases are significantly different from the baseline group as well as from each other according to Mann-Whitney U-tests tests (table 2). This result is also confirmed by the regression analysis (table 3): The last column (T123) combines observations from all treatments and shows positive, significant coefficients for unrelated and coordinative communication, which predict an even larger effect in the multivariate model than the simple differences in the average contributions presented in table 2.

> *Result 1: Non-coordinative communication has a significant, positive effect on cooperation and amounts up to roughly half of the total effect of coordinative communication.*[43]

[43] According to the regression coefficients, the effect of non-coordinative communication amounts up to 51.28% of the total communication effect with coordinative discussions, whereas in a direct comparison of average increases we are looking at 27.18%.

Table 3: Regression models for contribution to public good

dependent variable: Contribution	**T1** no comm.	**T2** unrelated	**T3** coordinative	**T123** combined
Expectation	0.047	0.301***	0.706***	0.385***
	(0.80)	(0.01)	(0.00)	(0.00)
Family	1.107***	-0.228	-0.318	0.044
	(0.00)	(0.52)	(0.39)	(0.84)
Friends	0.908**	0.143	-0.642	-0.015
	(0.04)	(0.71)	(0.10)	(0.95)
Age	0.036	0.020	-0.037	-0.001
	(0.38)	(0.66)	(0.10)	(0.95)
Female	-0.203	0.088	-1.342**	-0.723
	(0.84)	(0.92)	(0.04)	(0.19)
Schooling_years	-0.202	-0.073	-0.020	-0.131*
	(0.29)	(0.52)	(0.86)	(0.07)
Head_of_hh	-0.852	0.796	0.118	-0.037
	(0.48)	(0.38)	(0.87)	(0.94)
Migrant_10	1.789	0.438	0.072	1.002
	(0.18)	(0.69)	(0.91)	(0.10)
Social_ladder	-0.096	-0.055	0.032	0.035
	(0.63)	(0.68)	(0.79)	(0.72)
Cattle_owned	0.012	0.011	-0.001	0.001
	(0.81)	(0.62)	(0.97)	(0.91)
T_Unrelated				1.313**
				(0.04)
T_Coordination				2.467***
				(0.00)
_cons	0.684	1.952	3.905*	1.618
	(0.79)	(0.44)	(0.06)	(0.22)
N	52	55	65	172
F	3.90	4.98	7.66	4.94
p>F	0.007	0.002	0.000	0.000
R2	0.256	0.235	0.539	0.264

p-values in parentheses
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculations based on collected data

5.2 Group composition and social ties

The group composition measuring pre-existent social ties only shows a significant impact in the baseline treatment (T1). Both the share of family members and the share of friends in the group lead to higher contributions to the public good. Interestingly though, they do not seem to affect decisions to cooperate anymore in either communication treatment (T2 and T3).

> *Result 2: Previously existent social ties have a positive effect on cooperation outcomes but only in the condition without communication.*

5.3 Expectations of others' contributions and trust in group members

In order to find out more about the reasons for the increases in cooperation after communication, I now look at the beliefs about the other group members' contributions. As visible in figure 5, there is very little variation in average belief across all treatments. Mann-Whitney U-tests of all treatments against each other, confirm that the differences are not significant (table 2).

Figure 5: Average Expectations and Trust[41]

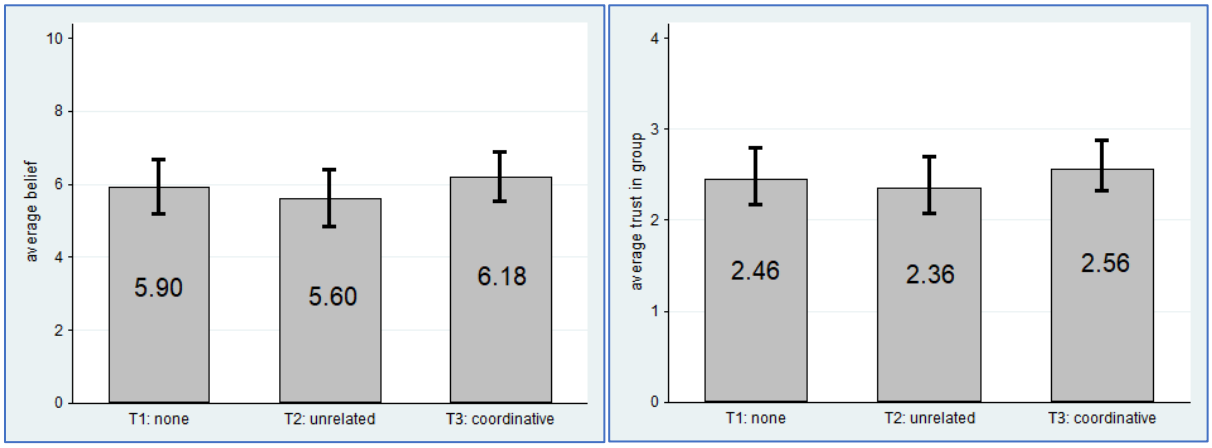


Table 4: Regression models for expectations about others' contributions

dependent variable:	**T1**	**T2**	**T3**	**T123**
Expectation	no comm.	unrelated	coordinative	combined
Family	0.212	-0.624	-0.189	-0.157
	(0.37)	(0.16)	(0.64)	(0.48)
Friends	0.092	-0.261	0.127	0.070
	(0.87)	(0.58)	(0.76)	(0.81)
Age	-0.014	0.033	0.001	0.005
	(0.67)	(0.42)	(0.95)	(0.78)
Female	-0.568	1.663**	-1.638*	0.127
	(0.45)	(0.02)	(0.07)	(0.82)
Schooling_years	-0.054	0.085	-0.144	-0.036
	(0.73)	(0.44)	(0.19)	(0.64)
Head_of_hh	0.778	-0.249	-1.467*	-0.031
	(0.47)	(0.75)	(0.08)	(0.95)
Migrant10	-1.885**	1.924**	0.530	0.420
	(0.04)	(0.05)	(0.58)	(0.49)
Social_ladder	-0.024	-0.209	0.096	-0.136
	(0.83)	(0.24)	(0.55)	(0.21)
Cattle_owned	-0.028	0.002	0.014	0.006
	(0.54)	(0.89)	(0.35)	(0.58)
T_Smalltalk				-0.119
				(0.88)
T_Coordination				0.244
				(0.74)
_cons	6.904***	4.244*	8.364***	6.263***
	(0.00)	(0.07)	(0.00)	(0.00)
N	52.000	55.000	65.000	172.000
F	1.54	5.79	1.42	0.72
P > F	0.212	0.002	0.255	0.716
R2	0.138	0.287	0.146	0.035

p-values in parentheses
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculations based on collected data

Results from regressions on expectations as the dependent variable are in line with results from the tests (table 4). The regression models further reveal that social ties do not affect expectations in any treatment condition. The variability for expectations, measured as standard deviation, is not different across treatments, either (table 2). It stands out, however, that average beliefs about the other players' contributions are higher than the actual average contributions, especially in treatment 1 and treatment 2, which means that, on average, players deliberately contributed less than what they expected their group members to contribute (table 2).[44]

I also measured the correctness of stated beliefs: In treatment 3 more than half of the participants (55%) correctly guessed the average contribution of their group members compared to 29% and 36% for treatment 1 and 2, respectively. Within-group variation of contributions and expectation was also much smaller (table 2) in the coordinative communication condition, i.e., behavior was more homogenous in groups, which is likely a result of discussions and agreements on a certain amount to contribute.

Individual expectations were also added as an explanatory variable in the main regression table (table 3). This is possible as it has just been shown that there is no correlation between expectations and treatments conditions. It turns out that, on individual level, expectations do have a positive and significant correlation with contributions in both communication treatments (T2 and T3) and the combined regression model (T123), but not in the no-communication condition (T1) (table 3). This finding corroborates the concept of conditional cooperation and, in turn, demonstrates that measurement of expectations was not defective in my study.

Similarly, trust in one's group members as elicited in the survey questions is compared, but is not found to be affected by the treatment condition, either (figure 5 and table 2). With "3" being the highest possible value, perceived levels of trust were predominantly reported as rather high by the majority of participants.

> *Result 3: Expectations and trust are not affected by either type of communication treatment, but on individual level higher expectations correlate with higher contributions*

[44] This is, generally, not an uncommon finding. People are conditionally cooperative but usually try to stay below the groups' average contribution in order to benefit a bit more rather than take the risk of contributing more than average and thereby receiving a smaller payoff than the others (Fischbacher and Gaechter 2010).

5.4 Comprehension

Significantly more participants answered both control questions correctly after coordinative discussions in treatment 3 (90%) compared to treatment 1 (72%) and treatment 2 (76%), which shows that talking with one's group members about the social dilemma and game mechanics increases comprehension (table 2). However, comparing contributions between those, who have answered both control questions correctly, and those, who have not, clearly shows that contributions from the latter were higher in all treatments.[45] While taking into account that answering two control questions might not exactly reflect the level of comprehension for all participants, it should be safe to conclude that increased comprehension can be ruled out as the reason for higher contributions after coordinative communication.[46]

> *Result 4: Coordinative communication increases comprehension, but increased comprehension does not raise cooperation (on the contrary, better comprehension is rather associated with lower cooperation).*

6. Discussion

First of all, it could be shown that, contrary to a number of previous empirical findings, cooperation is raised through unrelated communication even without the possibility to coordinate (cf. Bouas and Komorita 1996; Bichieri and Lev-On 2007). As unrelated talk does not allow explanations, appeals or making commitments, increases in cooperation rates must be the result of changes in group members' relationship to each other, i.e., working over the non-coordinative channel. Following the theories presented in my framework, this particularly stresses the relevance of social distance and group identity as motivational factors which are affected by communication.

Coordinative communication, however, resulted in even higher rates of cooperation than talking about an unrelated topic. Concerning potential reasons for this finding, I can firstly rule out better comprehension: While coordinative communication indeed helped increasing

[45] But not significantly different from them, which is – despite high absolute differences – a consequence of the low number of participants who gave wrong answers.

[46] Presented average contributions are based on the cleaned data and do therefore not include observation from participants who misunderstood the game and the communication effect. They can hence not be influenced by those who answered the control questions wrongly, either way. It can be deducted however, that comprehension is not a binary measurement, but that even amongst those who answered the control questions correctly, degrees of comprehension vary and the direction of a potential effect on cooperation is equivalent.

comprehension of the dilemma, better comprehension could not be associated with higher contributions. Interestingly, neither trust in one's group members nor expectations of their contributions were affected by the different communication conditions, leading to the conclusion that neither the coordinative nor the non-coordinative communication raises cooperation primarily via trust and expectations (cf. Bornstein and Rapoport 1988).[47] If communication worked through affecting the next potential motivational factor in my model, social norms, it should, by all means, also be reflected in altered average expectations. In other words, one would not follow a social norm to cooperate without believing others do so as well. The same applies to not lying as a social norm, i.e., not breaking promises of commitment made in the discussion, as believing in one's group members' promises to cooperate would then also show in elevated expectations. This was, however, not found to be the case. This leaves us with three remaining, viable options for relevant motivational factors in the communication effect: Personal norms, group identity and social distance, whereof the latter two remain, as explained in my theory framework, somewhat empirically indistinguishable.

Concerning social distance, results have shown a significant effect in the baseline condition, where no communication is allowed: Both, the number of family members and the number of friends in one's group increased contributions to the public good, which means that there is some general, positive effect of social closeness on cooperation decisions and is in line with existing literature (Essock-Vitale and McGuire 1985; Yamagishi and Sato 1986; Thompson et al. 1998; Kollock 1998; Monsutti 2004; Peters et al. 2004; Bowles and Gintis 2004; Goette et al. 2006; Ruffle and Sosis 2006; Haan et al. 2006; Castro 2008; Boone et al. 2008; Apicella et al. 2012; Chuah et al. 2014). However, once participants are allowed to talk to each other, social ties do apparently not play a relevant role anymore. It seems as if effects of previously existent social ties are overridden by spontaneous alterations in intragroup relationships induced by communication. In other words, the effect of a short, and even unrelated, discussion on the relationship between participants is more important than previous, long-term social ties.

It was not found that a higher number of family members and friends in the group raises trust or expectations in any treatment condition. This means that rather than following a social norm or a feeling of having to meet certain expectations, contributing more in the presence of socially close group members is actually an independent, personal preference. Such can be explained by in-group favoritism, and also finds support in evolutionary theories on kin selection (Tajfel et al. 1971; Tajfel et al. 1979, Caporeal et al. 1989, Peters et al. 2004; Candelo et al. 2018)

[47] Bornstein and Rapoport (1988) found communication to increase expectations in a public good experiment.

So far, I have established that group members' relationship to each other affects the decisions to cooperate, both through long-term previously existent social ties and spontaneous changes induced by unrelated discussions. Coordinative discussions, as in treatment 3, do, however, result in even more cooperation than unrelated ones. Can this difference also be explained by further changes in group members' relationship to each other?

It could be hypothesized that finding agreement on an important, relevant topic, such as the mutual consent to cooperate, could evoke an even stronger group identity and reduce social distance further than just talking about unrelated issues (Dawes et al. 1988; Orbell et al. 1988; Kerr and Kaufman-Gilliland 1994; cf. Bouas and Komorita 1996; Bicchieri 2002; Spears 2011). [48] , [49] However, Bouas and Komorita (1996) experimentally tested if unrelated communication creates group identity and could not find any empirical support for this theory.

While this option cannot be entirely ruled out on the basis of my empirical observations, I present, in the following, a better explanation for the additional increase in cooperation after coordinative discussion. This explanation is based on personal norms, which are the last remaining factor according to my framework and will turn out as perfectly in line with results from previous studies. The relevant personal norm in this situation is the one to fulfill one's own appeals and promises to contribute made during discussion, despite not necessarily believing in one's group members' pledges. In other words, individuals feel bound to stick to their commitments not because these were made as social contracts, but due to their own standards and norms of appropriate conduct. This explanation is in accordance with existing literature on the communication effect which found the adherence to commitments rather than the creation of a general norm to cooperate as the central reason for increased cooperation after communication (Orbell et al. 1988; Ostrom et al. 1992; Kerr and Kaufman-Gilliland 1994; Bicchieri 2002). My interpretation is further corroborated by results from Vanberg (2008), who analyzed the motives for promise keeping in general and distinguishes the preference for keeping promises per se from the motivation to not disappoint those who one has made such promises to. Based on results from a dictator game experiment Vanberg found that promises are kept due to the personal feeling of being obliged to do so, and not in order to avoid letting others down. Now, what does this result imply for theories explaining decision making in social

[48] According to this theory, unrelated discussions about agriculture and climate would have led to consent on this topic, thereby increasing group identity. This effect would be even stronger in the coordinative discussions, resulting in mutual agreement that contributing is the preferable option (even independent from commitments made and actual decisions). Possibly, a discussion about something controversial like politics or religion could have led to disputes and consequently a decrease in cooperation. This is an interesting aspect for further research.
[49] Spears (2011) point out that group identity can become salient depending on the content of the conversation in group discussion.

dilemma situations? While my data generally confirm the concept of conditional cooperation, I have found evidence that decisions are individual, independent preferences and not based on the perception of being bound to fulfil social norms or to meet others' expectations.

Two reasons were brought forwards to support this proposition: Firstly, there was no effect of either communication condition on trust and beliefs, but nonetheless communication led to higher cooperation rates. Secondly, and similarly, there was a preference to cooperate more with socially close group members, but no association with raised trust or expectations, either.[50]

As an alternative interpretation, which would have similar effects and consequences, it could be suggested that is it not the deliberate adherence to a personal norm of keeping promises but an intuitive, and possibly irrational, stickiness to commitments made during discussions (Orbell et. al. 1988; Kahnemann 2011). What speaks against this hypothesis is that previous experimental studies have found no effect of signaling one's intentions without the possibility to freely discuss the dilemma (Dawes 1977; Chen and Komorita 1994; Wilson and Sell 1997; Bochet et al. 2006). If there is no effect of (non-binding) pledges without communication, it can be deduced that they only work if participants have attained a certain level of relationship that make the norm of promise keeping salient in the respective context.[51] In other words, personal norms of keeping promises are potentially not important towards socially distant partners (Hoffman et al. 1996; Hardin 2003; Simspon 2007; Barbalet 2009).

7. Conclusion

Results show that discussions are effective through both altering the relationship between participants and coordination about the dilemma. Indeed, communicating about an unrelated topic already led to an increase in cooperation that amounts up to nearly half of the effect of coordinative communication. After ruling out alternative explanations, it could be concluded that the effect of coordination primarily originates from making commitments, which is supported by previous empirical studies on the topic. Surprisingly though, evidence at hand reveals that these commitments are effective not through raising expectations about each other's

[50] As implicated before, errors in measurements seem highly unlikely as both, variables for trust and expectations, were collected independently and expectations explicitly incentivized in order to make statements more salient. Also, expectations were found to confirm concept of conditional cooperation, thus supporting their validity.

[51] While I did not test a condition that allows signaling one's intentions without any free communication (which would have constituted the counterpart to the unrelated communication) I can draw from the plentiful evidence that previous literature provides (see sources above). A suggestion for future research could be such a condition with participants that (partly) know each other and analyzing whether non-binding pledges are more effective with socially close group members. According to the theories derived from my results, this should be the case.

behavior, but through the personal norm of complying with one's own commitments made during discussions.

The shift of focus from adherence to the (possibly not effective) social norms of cooperating to the personal norm to keep promises might be an interesting tool for achieving higher cooperation outcomes. It can be assumed that personal norms work in certain situations independently from social norms and expectations about others' compliance.

However, considering previous studies that did find non-binding pledges alone as insufficient to increase cooperation, it can be derived that commitments do not work in every situation, but require a certain level of interpersonal relationship amongst group members as accomplished, for example, by face-to-face conversations, in order to activate personal norms of promise keeping. Strengthening group members' relationship to each other is, therefore, another relevant element that is conducive to cooperation in groups. This could be unambiguously proven by my results, which show higher cooperation with socially close group members as well as significant increases achieved by unrelated discussions.

To conclude, my study has contributed to the disputed debate about whether communication in social dilemma situations increases cooperation through commitments or strengthening of interpersonal relationships and finds both elements to be relevant. Taking into account, however, that average expectations about one's group members' contribution were above one's own contributions in all treatments, and especially so without communication, it should perhaps be worded differently: Individuals behave less selfishly after communication.

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Supplementary Materials Chapter IV

A. Appendix

A.1 Map of selected villages

A.2 Table for split sample of treatment groups

A.3 Post-regression testing

B. Experimental Protocols

B.1 Village meeting

B.2 General instructions

B.3 Game instructions public good game

B.4. Examples

B.5 Control questions for public use

B.6. Treatment conditions

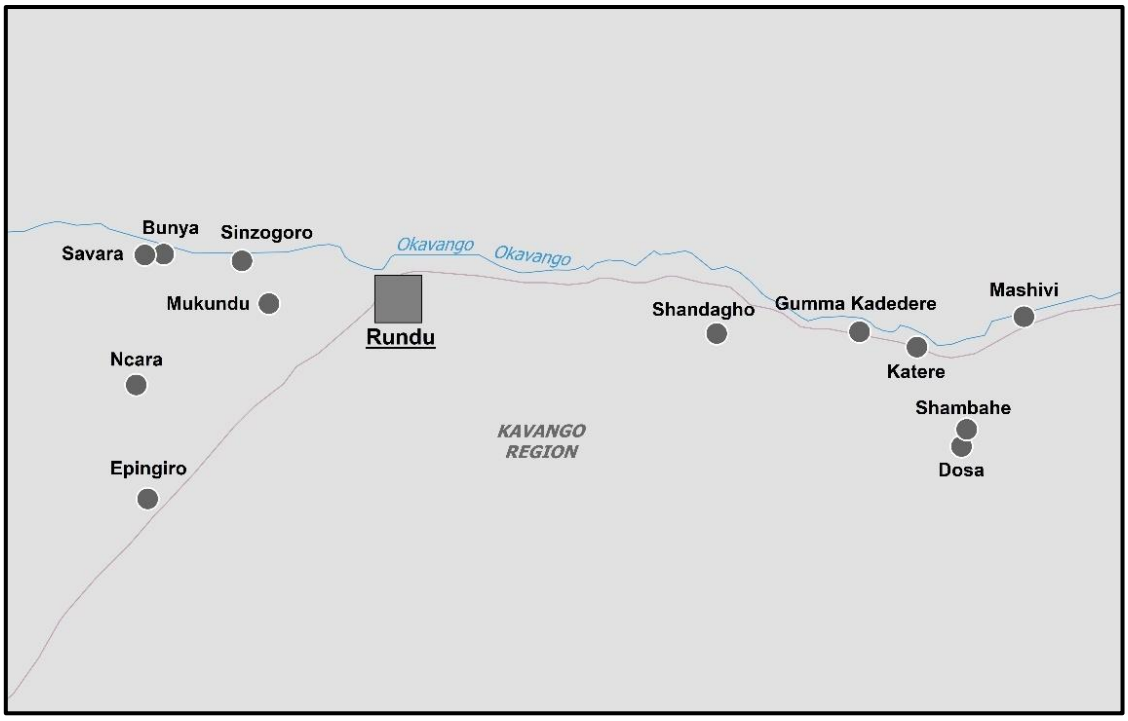
B.7 Decision making

C. Survey Questions

D. Information for data and analysis script request

Supplement A: Appendix

A.1 Map of selected villages

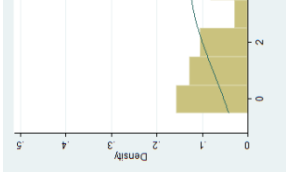
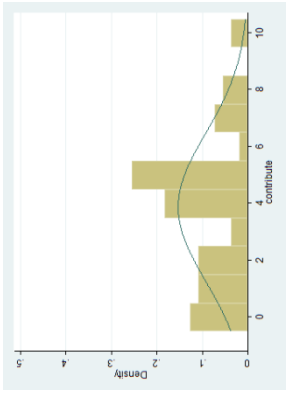
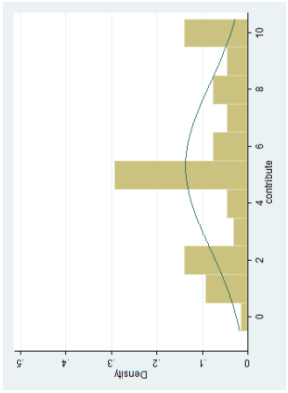
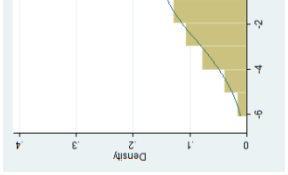
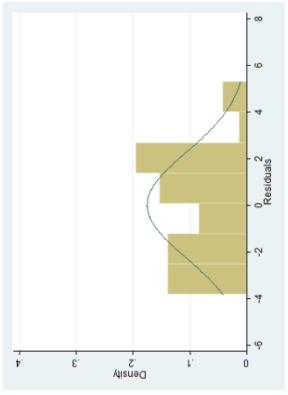
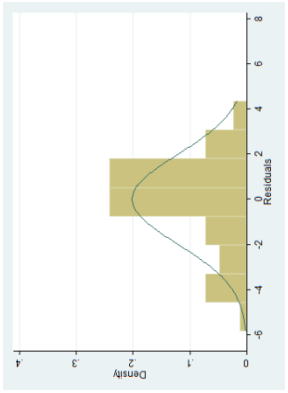
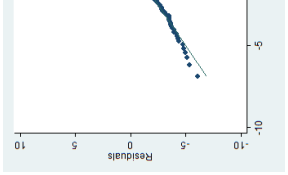
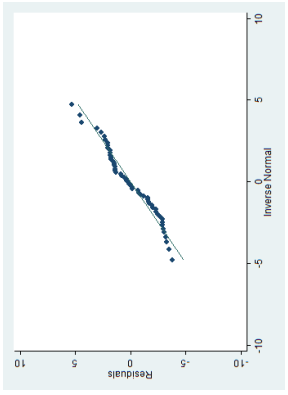
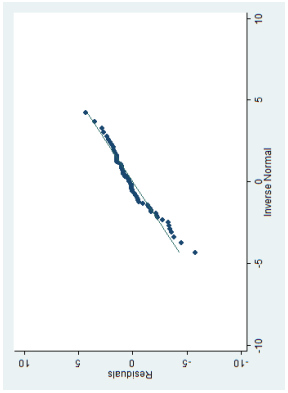


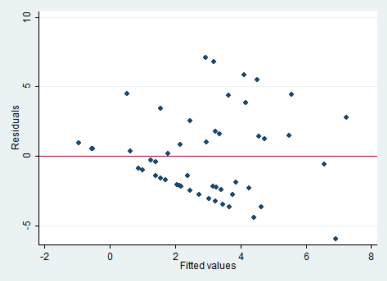
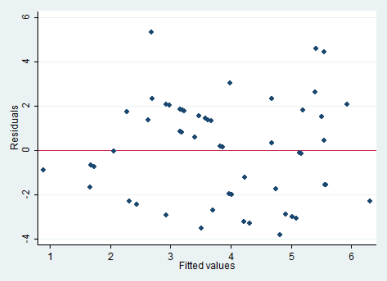
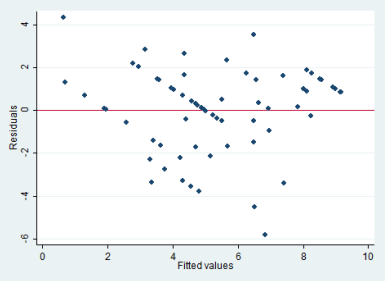
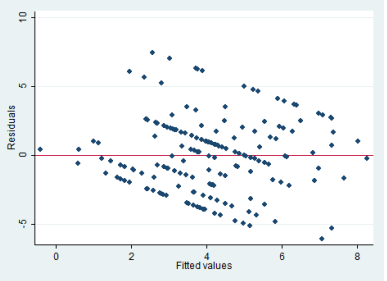
A.2 Table for split sample of treatment groups

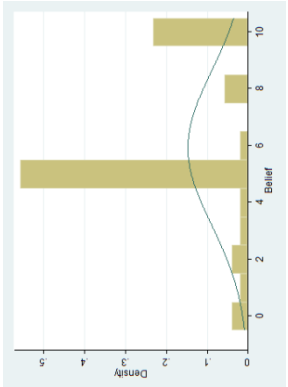
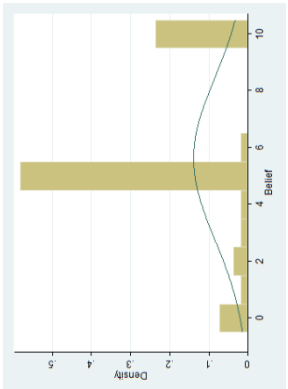
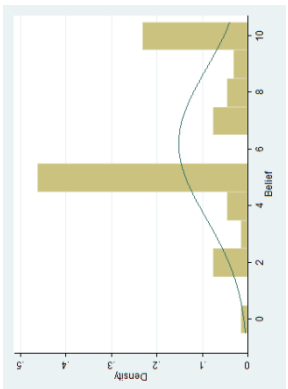
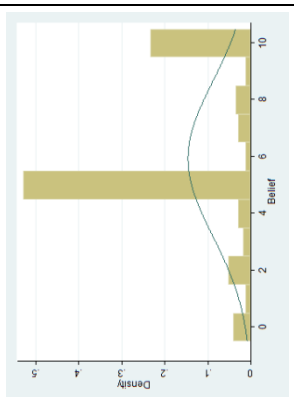
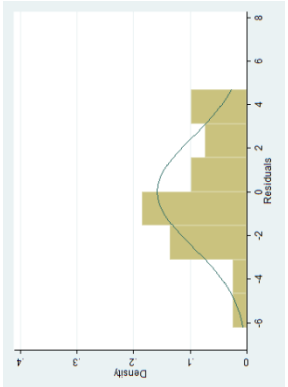
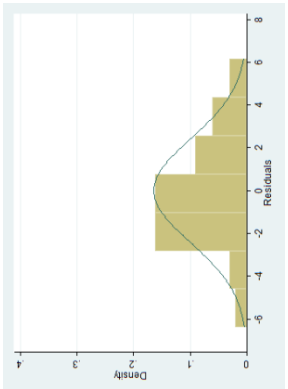
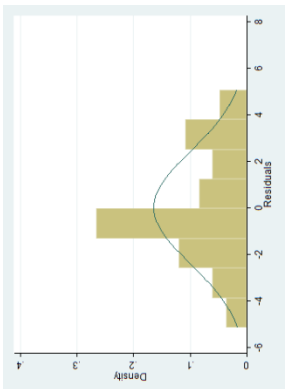
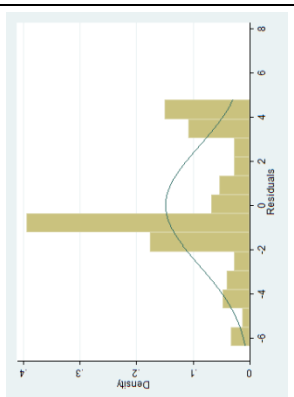
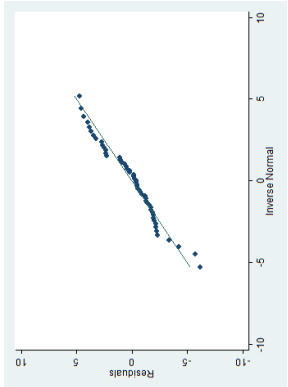
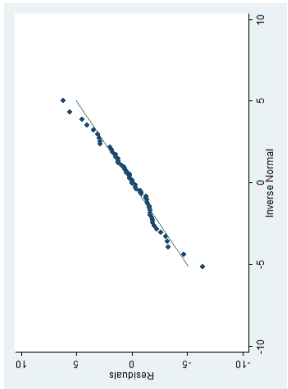
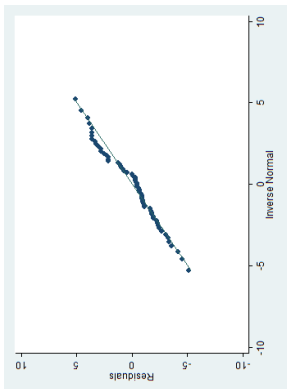
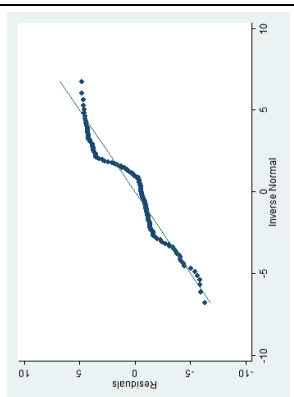
Treatment Number:	(T1) no comm. mean	(T2) unrelated mean	(T3) coordnative mean	Test for equality p-value
Family	1.42	1.42	0.82	0.0123**
Friends	0.77	0.58	0.71	0.8719
Age	38.73	32.71	36.58	0.1877
Female	0.42	0.67	0.68	0.008***
Schooling_years	6.69	7.73	7.03	0.1043
Head_of_hh	0.54	0.36	0.35	0.088*
Migrant10	0.21	0.15	0.18	0.669
Social_ladder	2.17	3.24	2.72	0.1128
Cattle_owned	6.60	11.45	9.28	0.4186
Observations	52	55	65	

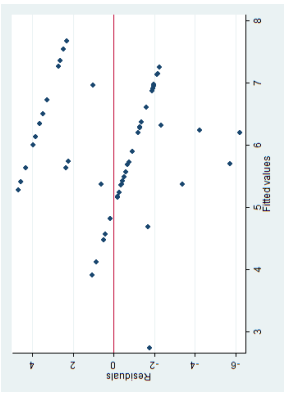
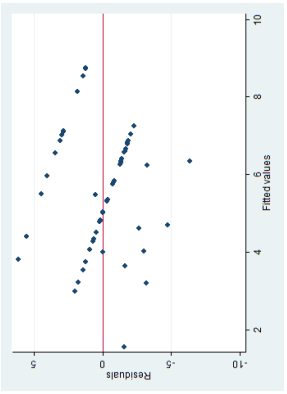
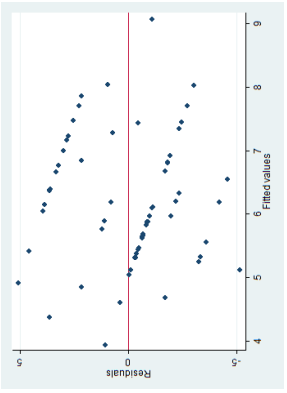
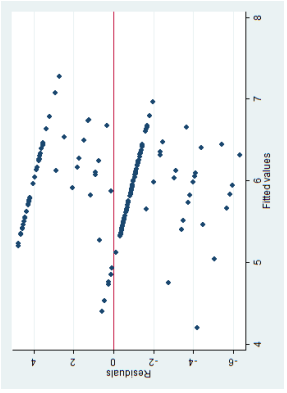
P value for Kruskal-Wallis test or Chi2-test in case variable value is binary
Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Source: own calculation based on collected data

A.3 Post-regression testing

Contribution	T1: no communication	T2: unrelated	T3: coordinative	T123: combined
Distribution of dependent variable				
Distribution of residuals				
Qnorm plot for residuals				

Contribution	T1: no communication	T2: unrelated	T3: coordinative	T123: combined
rvfplot				
VIF	Variable | VIF 1/VIF Age 2.44 0.409302 Head_of_hh 2.26 0.441643 Schooling_~s 1.91 0.524490 Female 1.77 0.564345 Family 1.67 0.599537 Cattle_owned 1.54 0.649581 Friends 1.47 0.682338 Migrant10 1.37 0.731843 Social_lad~r 1.30 0.767035 Expectation 1.16 0.862239 Mean VIF 1.69	Variable | VIF 1/VIF Age 2.00 0.499036 Head_of_hh 1.57 0.635091 Family 1.47 0.681495 Schooling_~s 1.45 0.687523 Friends 1.41 0.709413 Expectation 1.40 0.713494 Female 1.36 0.732742 Migrant10 1.32 0.757615 Social_lad~r 1.28 0.779150 Cattle_owned 1.22 0.820893 Mean VIF 1.45	Variable | VIF 1/VIF Head_of_hh 1.80 0.556764 Age 1.52 0.657306 Female 1.51 0.663657 Schooling_~s 1.38 0.722759 Friends 1.32 0.755935 Cattle_owned 1.21 0.827356 Social_lad~r 1.18 0.846297 Expectation 1.17 0.854262 Family 1.16 0.858418 Migrant10 1.15 0.869105 Mean VIF 1.34	Variable | VIF 1/VIF Age 1.77 0.566473 Head_of_hh 1.65 0.607331 T_Coordina~n 1.57 0.638290 T_Smalltalk 1.55 0.643193 Female 1.33 0.750691 Family 1.33 0.752019 Schooling_~s 1.33 0.754024 Friends 1.21 0.826354 Social_lad~r 1.16 0.860281 Cattle_owned 1.14 0.874912 Migrant10 1.12 0.896089 Expectation 1.04 0.965006 Mean VIF 1.35

Expectation	T1: no communication	T2: unrelated	T3: coordinative	T123: combined
Distribution of dependent variable				
Distribution of residuals				
Qnorm plot for residuals				

Expectation	T1: no communication	T2: unrelated	T3: coordinative	T123: combined
rvfplot				
VIF	Variable | VIF 1/VIF Age | 2.44 0.410653 Head_of_hh | 2.24 0.446418 Schooling_~s | 1.90 0.525981 Female | 1.76 0.568410 Family | 1.66 0.603698 Cattle_owned | 1.53 0.654608 Friends | 1.46 0.683078 Social_lad~r | 1.30 0.767239 Migrant10 | 1.27 0.786804 Mean VIF | 1.73	Variable | VIF 1/VIF Age | 1.98 0.505488 Head_of_hh | 1.57 0.636093 Schooling_~s | 1.43 0.697017 Friends | 1.40 0.713706 Family | 1.37 0.727284 Female | 1.26 0.794080 Migrant10 | 1.24 0.806282 Social_lad~r | 1.23 0.813340 Cattle_owned | 1.22 0.821027 Mean VIF | 1.41	Variable | VIF 1/VIF Head_of_hh | 1.71 0.584473 Age | 1.52 0.657325 Female | 1.41 0.711618 Schooling_~s | 1.34 0.745190 Friends | 1.32 0.757495 Cattle_owned | 1.20 0.835916 Social_lad~r | 1.17 0.852249 Family | 1.16 0.862887 Migrant10 | 1.14 0.874668 Mean VIF | 1.33	Variable | VIF 1/VIF Age | 1.76 0.566685 Head_of_hh | 1.65 0.607344 T_Coordina~n | 1.56 0.639092 T_Smalltalk | 1.55 0.643373 Female | 1.33 0.750996 Family | 1.32 0.754722 Schooling_~s | 1.32 0.755421 Friends | 1.21 0.826802 Social_lad~r | 1.15 0.871168 Cattle_owned | 1.14 0.876117 Migrant10 | 1.11 0.899041 Mean VIF | 1.37

Supplement B: Experimental Protocols

B.1 Village meeting

[VILLAGE MEETING]

[freely presented by Christian, interpreted by Moses (assistant)]

To begin with, we would like to thank you all for coming here today. My name is Christian Hoenow. I am from the Marburg University in Germany. Together with the Ministry of Agriculture, Water and Forestry we are conducting research under the SASSCAL project. [NAME OF EXPRIMENTERS] are also part of the project.
Doing research means we are just here to collect data, but we do not bring any type of development project into the village. What you answer in the workshop will not have any impact on future projects.
Today we would like to conduct two small workshops with a certain number of people. During this workshop we will also ask you several questions. Unfortunately, not everyone from this village can participate since each workshop can only include a certain number of participants.
Since we want everyone to have the same chance to participate, we have prepared a bag with as many cards as people present. Each adult that is older than 18 years now will draw a card. We will ask you to fully concentrate on the workshop and we will be asking many questions. If you already know that you cannot attend for up to 5 hours, or do not wish to answer many questions, you should please not draw. Participation is, of course, voluntary!

- If you draw a red card, you will participate in the first workshop, which is conducted by Christian
- If you draw a blue card, you will participate in the second workshop, which is conducted by Adrian
- If you draw a white card, you unfortunately cannot participate in any of the events.

Do you have any questions?

[let every adult draw a card]
Now that everyone has drawn a card, we would like to ask all participants with a white card to leave the area. Thank you very much for attending the meeting.

Those who have drawn a blue card, please follow Adrian. He will right away start with the meeting.

[wait for everyone to leave except Christian's participants]

[continue with general instructions in each group]

B.2 General instructions

[GENERAL INSTRUCTIONS]

To begin with, we would like to thank you all for coming here today.
We will conduct a workshop and at the end you will receive some payment for participating. The money is not our private money, but it is provided by the German government.
All information collected today will be used for research only. Neither the government of Namibia, Germany nor any other organization will receive the data for other purposes. Also, neither your names nor any village-specific information will be linked to the results. All answers will remain anonymous to others.

The schedule for today looks as follows:

1. We will explain the procedure of the workshop.
2. We will conduct a small workshop.
3. After the games each of you answers a short questionnaire.
4. Finally, you will receive the money.

Before starting, I would like to give you some general information:

1. If at any time, you think that this is something that you do not wish to participate in for any reason, you are free to leave. You will however only receive a payment if you stay until the end of the workshop.
2. If you already know that you will not be able to stay for at least 5 hours, then you should leave right away.
3. We require your complete and undistracted attention. Please, follow the instructions carefully and do not use your phone or engage in any other distracting activity.
4. It is not allowed to talk to each other during the workshop, unless we tell you to. You can ask questions after raising your hand. If you talk to each other when you are not allowed to, you will be excluded from the workshop and the payments.
5. Everyone of you has received a unique ID card. Please keep this ID until the end. You must return the ID before receiving the money at the end of the workshop.

After knowing these rules, is there anybody who does not want to participate?
Do you have any questions?

[continue with treatment conditions]

B.3 Game instructions public good game

[COMMUNICATION GAME INSTRUCTIONS]

We will now explain the procedure of the workshop. Please pay attention as for participating it is necessary that you understand everything. Also, we will later ask you questions individually to check whether you understood everything correctly. Each one of you will now receive an envelope that contains 10 Experimental Coins (EC). Each EC is worth 5 N$. **[show coins]**. You will have to decide whether to contribute that money to a group account or not. What you put in the brown envelope is what you want to contribute to the group account, whereas what you wish to keep must be put in the white envelope **[show envelopes]**.

You can contribute any amount between 0 and 10 EC. The coins that you do not contribute are yours and you can keep them for sure. After the game we will change them for you: 5N$ for every EC. [See graph with exchange rate] In total you can get between 25 and 125N$, depending on your decisions and the decision of the others players in your group. You are playing the game with three other players, i.e., in groups of four.

- The number of ECs that were contributed to the group account are doubled. This doubled amount is then equally divided by all four players in the group.
- That means every player receives on fourth of the doubled group account.
- In total you will earn the ECs that you keep plus the share that you receive from the group account
- Note that the game is not about luck and not about being better than others. Everyone will receive exactly the amount as determined by the rules explained.

B.4 Examples

Example 1

[use poster and fill with example numbers]

4 players contribute half of their endowment to the group account.

- There are then 5 x 4 = 20EC in the group account.
- The 20EC in the group account are then doubled (20EC x 2 = 40 EC) and divided equally to all 4 players.
- This means each player in the group receives 40EC / 4 = 10 EC from the group account.

Each player then ends up with the amount that he/she kept, which is 5EC and the amount that he/she received from the group account, which is 10EC. In total it results in 15EC for all players.

Example 2

[use poster and fill with example numbers]

3 players contribute all of their ECs to the group account and 1 player does not contribute anything.

- There are then 30EC in the group account.
- The 30EC in the group account are then doubled (30EC x 2 = 60 EC) and divided equally to all 4 players.
- This means each player in the group receives 60EC / 4 = 15 EC from the group account.

The one player that did not contribute receives 15EC from the group account plus 10EC that he/she kept for himself/herself, which is 25 EC in total.
The three players that contributed everything receive 15 EC each from the group account.

Example 3

[use poster and fill with example numbers]

Imagine now that the one player also contributes. So, everyone contributes everything.

- Then the total contributions are 4x10 = 40. Multiplied by 2 = 80. 80 divided equally amongst all four players is 20EC for everyone.

- Then the three players get 20 instead of 15, and the one player who now also contributed also receives 20, instead of the 25 he/she would receive if NOT contributing.

B.5 Control questions for public use

We would now like to ask you a few questions to check if everybody understood:
[try to involve all participants]

1. If no one contributes anything, that means everyone keeps his/her initial ECs. Then how much does every player end up with?
   [10]

2. If everyone contributes all of his/her initial ECs, then how much does every player get? **[20]**

3. Are the payoffs for everyone higher, lower or the same if all 4 players contribute 8EC, compared to when all players contribute 5EC?
   [higher]

4. If you do not contribute anything are your own payoffs higher, lower or the same compared to when you contribute?
   [always higher]

5. What is your payoff if you contribute all of your 10EC but no one else contributes anything?
   [then only 10EC in the group account, 10*2 = 20, divided by four = 5ECs for everyone. Since you did not keep any of your initial EC, your final payoff is 5EC.]

6. How much does everyone else receive in this case?
   [keep 10 for themselves + get 5 from your contribution = 15]

7. If you end up with 10 coins, how many N$ will you get for that later?
   [5 x 10 = 50 N$]

- Very good. Is there anything unclear about the rules or how the payoffs are calculated?

- Should we have another example?

B.6 Treatment conditions

[TREATMENT 1: ANONYMOUS GROUPS]

[read Game Instructions]
In the following game you will be playing in groups of 4 players. As you see, we are 12 players here. Therefore, we will have 3 groups playing the game simultaneously. But you do not know who your three team members are. It will not be revealed after the game is over either. The other groups are playing the same game, but what they do does not influence your group or your payoffs. They do just play the same game simultaneously. The group allocation is entirely random according to your ID numbers.
Remember that you are not allowed to talk to each other

[TREATMENT 2: GROUP IDENTITY REVEALED]

[read Game Instructions]
In the following game, we will divide you into groups of four. That means each of you is playing the game with three other players. The other groups are playing the same game, but what they do does not influence your group or your payoffs. They do just play the same game simultaneously.

[allocate groups]

[groups should sit together, but keep distance to avoid communication between groups]
This is the group you will be playing with. Remember that you are not allowed to talk to each other.

[then wait for 2 minutes in silence before starting the game]

[TREATMENT 3: UNRELATED TALK]

[allocate groups]

[groups should sit together, but keep distance to avoid communication across groups]
We ask you to now please talk to each other for 5 minutes with your group members about how the different weather in this and in the last years affected the harvest. Also, think about which types of crops are doing good and which are doing bad in the different weather conditions.

[move away, wait 5 minutes, return to groups]
Please stop talking now as we are going to explain the rules of the game to you.
We will now explain what we are going to do in the workshop, you will be doing the workshop in groups of four, i.e., with the same 3 people that you just talked with.
The other groups are playing the same game, but what they do does not influence your group or your payoffs. They do just play the same game simultaneously.

[continue with game instructions]

[after reading out game rules and examples note this:]
your group is the group as allocated in the beginning during the discussions!

[TREATMENT 4: COORDINATION]

We will now explain to you the rules of the game.
In the following game we will divide you into groups of four. That means each of you is playing the game with three other players. The other groups are playing the same game, but what they do does not influence your group or your payoffs. They do just play the same game simultaneously.
[read Game Instructions]

[allocate groups]

[groups should sit together, but keep distance to avoid communication across groups]
You are now allowed to talk to your group members for 5 minutes. After the 5 minutes you will make your decisions in private. You may talk about anything you like.

[move away, wait 5 minutes, return to groups]
Please stop talking now as we are going to start with the decisions.

B.7 Decision making

[DECISION MAKING]

Your contribution will not be disclosed to the other participants. You will find out about the total contributions in your group at the end when we pay you, but no one will find out about how much other single players contributed. That means your own contribution is also anonymous to the other players. We will not disclose your decisions and you are under no obligation to tell anyone about how much you contributed.

In order to ensure anonymity in decision making, you will one-by-one come to the booth and make the decision there, in private. Please do not show other players how much you contributed, also not after you have made your contribution decision.

[show both envelopes and how to do it]

- Are there any questions about the procedure?

Before we start with the decisions, we would like to ask you two control questions, in order to check whether you have really understood the game. The answers you give here will not affect the money you earn, it is just for us as additional feedback information. [Assistant (me)] will ask you these questions, then you go directly to the booth and make your decision, then put the brown envelope, which contains your contribution to the group account into the box.

Please now come to the booth. We will call you one by one. Please remember to not talk to each other or communicate in any other way while waiting until everyone has made their decision. Also remember that there is no right or wrong in this game.

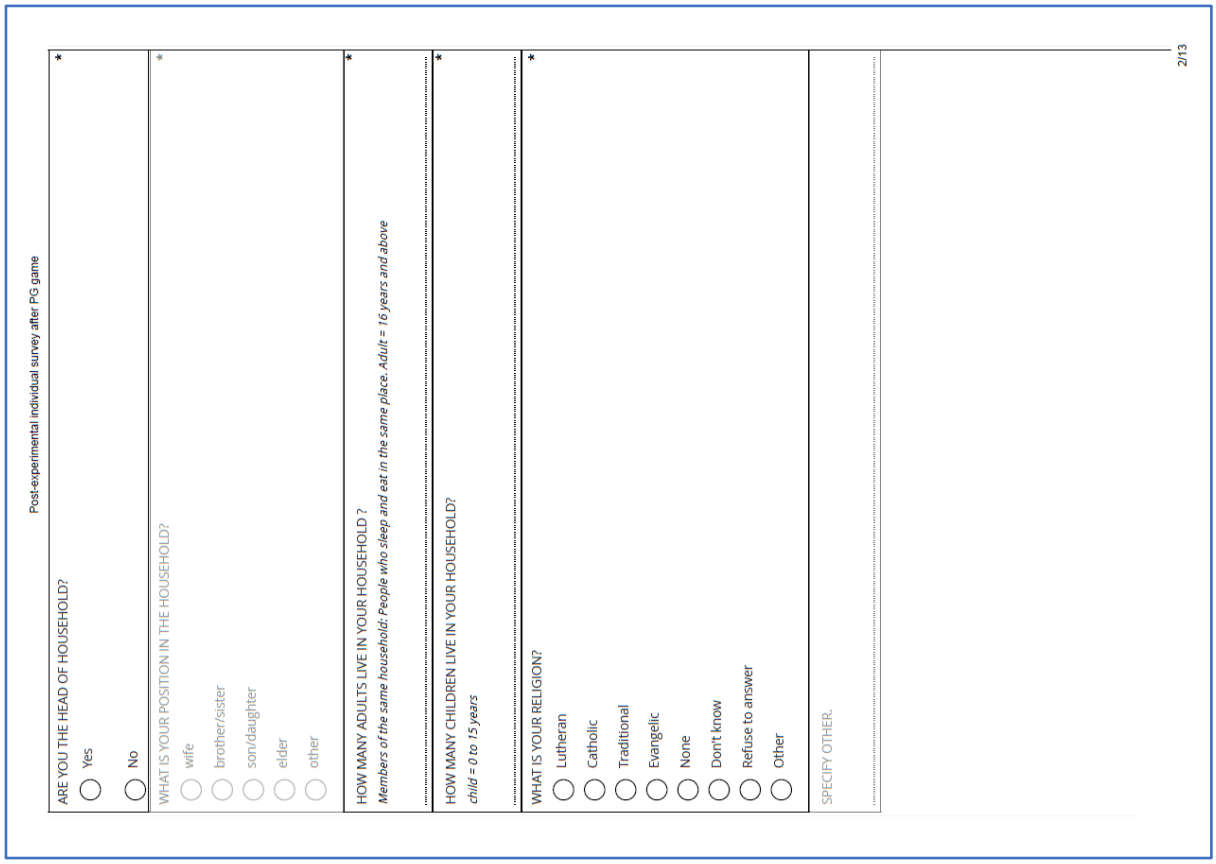
After the decision you may directly move to the snack area. There, you may talk again freely.

[one-by-one to assistant to answer two control questions in private, then to booth to make decision, in convenient order]

[have a break with snacks and cold drinks for everyone]

Thank you all for participating. You will now answer some short questionnaire and afterwards you will get the payments.

Supplement C: Survey Questions (Sheets made with Kobo-Toolbox: "https://www.kobotoolbox.org/")



Post-experimental individual survey after PG game

ARE YOU THE HEAD OF HOUSEHOLD? *
- Yes
- No

WHAT IS YOUR POSITION IN THE HOUSEHOLD? *
- wife
- brother/sister
- son/daughter
- elder
- other

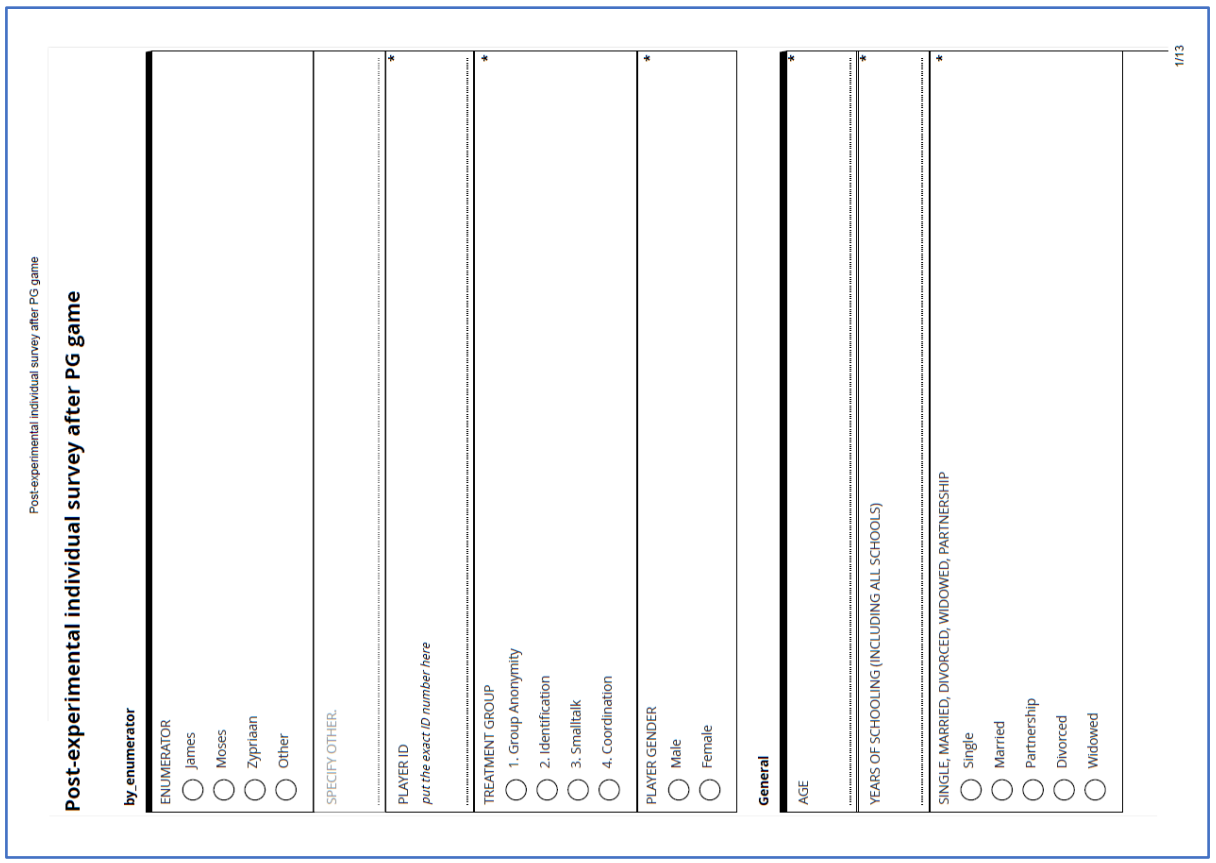
HOW MANY ADULTS LIVE IN YOUR HOUSEHOLD ? *
Members of the same household: People who sleep and eat in the same place. Adult = 16 years and above

HOW MANY CHILDREN LIVE IN YOUR HOUSEHOLD? *
child = 0 to 15 years

WHAT IS YOUR RELIGION? *
- Lutheran
- Catholic
- Traditional
- Evangelic
- None
- Don't know
- Refuse to answer
- Other

SPECIFY OTHER.

2/13



Post-experimental individual survey after PG game

Post-experimental individual survey after PG game

by_enumerator

ENUMERATOR *
- James
- Moses
- Zypriaan
- Other

SPECIFY OTHER.

PLAYER ID *
put the exact ID number here

TREATMENT GROUP *
- 1. Group Anonymity
- 2. Identification
- 3. Smalltalk
- 4. Coordination

PLAYER GENDER *
- Male
- Female

General

AGE *

YEARS OF SCHOOLING (INCLUDING ALL SCHOOLS) *

SINGLE, MARRIED, DIVORCED, WIDOWED, PARTNERSHIP *
- Single
- Married
- Partnership
- Divorced
- Widowed

1/13

Post-experimental individual survey after PG game

YOUR MOTHER TONGUE? *

- ◯ English
- ◯ Portugues
- ◯ RuShambyu
- ◯ RuKwangali
- ◯ RuNyemba
- ◯ Gciriku
- ◯ Mbukushu
- ◯ Kimbundu
- ◯ Chokwe
- ◯ San
- ◯ Oshiwambo
- ◯ Nama/Damara
- ◯ Afrikaans
- ◯ Otjiherero

Anonymity_Relations

HOW MANY OF THE OTHER PLAYERS IN THE WORKSHOP ARE:

RELATIVES AND OR SOMEONE FROM THE SAME HOUSEHOLD *

FRIENDS (BUT NOT RELATIVES) *

ACQUAINTANCES *
(Mugeni)

UNKNOWN *

Group_Relations

HOW MANY OF THE OTHER THREE PLAYERS IN YOUR GROUP ARE:

RELATIVES OR SOMEONE FROM THE SAME HOUSEHOLD *

FRIENDS (BUT NOT RELATIVES) *

3/13

Post-experimental individual survey after PG game

ACQUAINTANCES *
(Mugeni)

UNKNOWN *

Methodical_Questions

WHICH OF THE FOLLOWING DID YOU TALK ABOUT WITH THE OTHER THREE GROUP MEMBERS DURING THE EXPERIMENT? *

- ☐ Greet each other
- ☐ Smalltalk: Agriculture
- ☐ Smalltalk: About participants
- ☐ Smalltalk: Joking
- ☐ Smalltalk: About money
- ☐ Expectations of the experiment
- ☐ Discuss how to decide
- ☐ Explain the rules to other participants
- ☐ Other

SPECIFY OTHER.

DO YOU THINK THE PAYMENTS OFFERED ARE LITTLE, FAIR OR TOO MUCH? (REAL PAYMENTS NOT MADE YET, BUT ROUGHLY KNOW THE MIN AND MAX AMOUNT TO EARN) *

- ◯ too much
- ◯ fair
- ◯ too little

WHAT DO YOU THINK IS THE REASON FOR CONDUCTING THESE GAMES?
DO NOT READ ANSWERS

- ☐ Qualify for village support
- ☐ find out about the community
- ☐ distribute money
- ☐ don't know
- ☐ Other

SPECIFY OTHER.

4/13

Post-experimental individual survey after PG game

General_Information

IS FARMING YOUR MAIN PROFESSION? *

- ◯ Yes
- ◯ No

HOW MANY BAGS OF CROP YIELDS DO YOU NORMALLY PRODUCE PER YEAR? (1BAG = 50KG) *

number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)

.....

HOW MANY BAGS OF YOUR PRODUCTION DO YOU SELL? (1BAG = 50KG) *

number of bags (or add unit if interviewee wants to answer in NAD or other unit instead)

.....

AND THAT IS HOW MUCH INCOME PER YEAR FROM SELLING FARMING YIELDS? *

income from the WHOLE year or season in NAD

.....

DOES YOUR HOUSEHOLD RECEIVE ANY REMITTANCES FROM PEOPLE WORKING ELSEWHERE? (E.G. IN RUNDU) *

- ◯ Yes
- ◯ No

IS YOUR HOUSEHOLD OR SOMEONE IN YOUR HOUSEHOLD RECEIVING ANY PENSIONS (E.G. OLD OR HANDICAPPED) *

- ◯ Yes
- ◯ No

TOTAL INCOME INCLUDING EVERYTHING YEARLY? IN NAD *

income from: farming, remittances, pensions and other income

.....

WHICH OF THE FOLLOWING ASSETS DOES YOUR HOUSEHOLD OWN? *

- ☐ electricity from powerline
- ☐ generator for electricity
- ☐ radio
- ☐ television
- ☐ refrigerator
- ☐ motorized vehicle
- ☐ bicycle
- ☐ phone

5/13

Post-experimental individual survey after PG game

WHAT MATERIAL IS YOUR HOUSE MADE OF? *

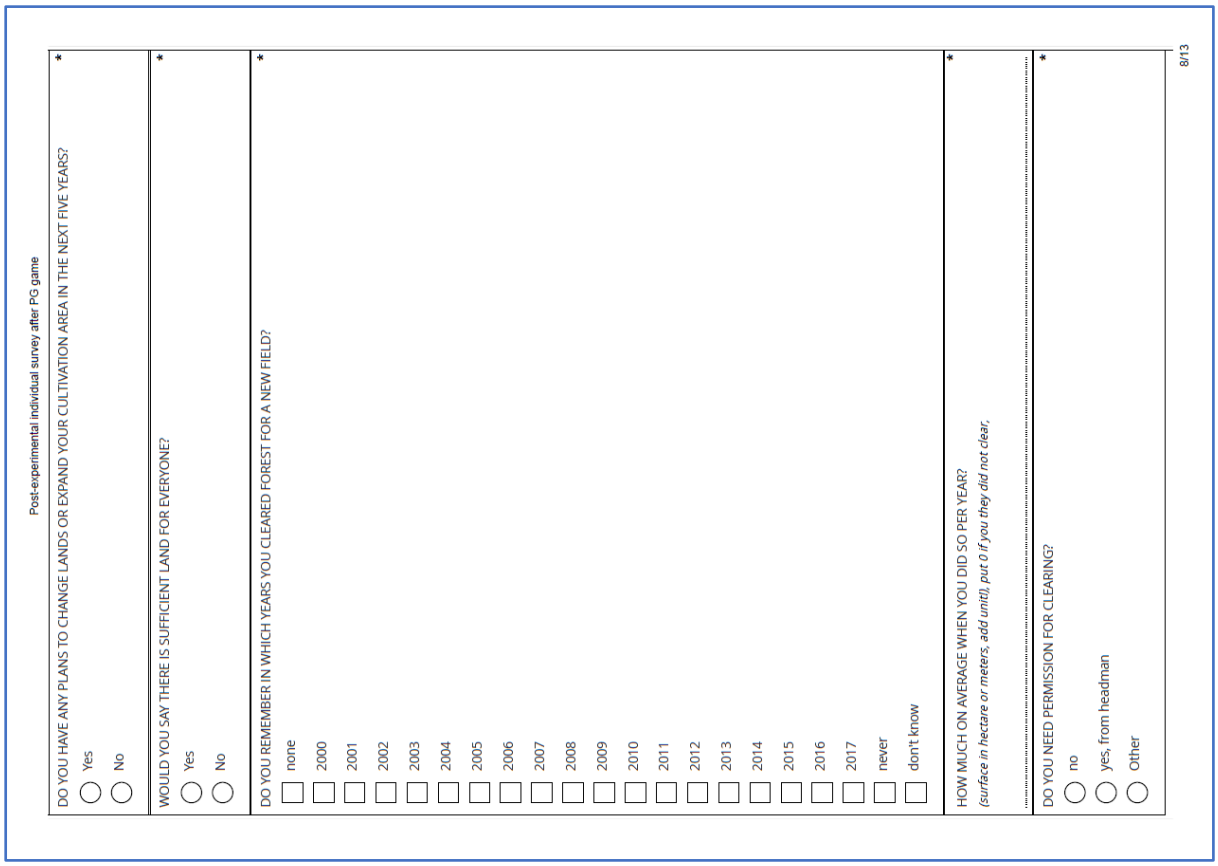
- ◯ tent
- ◯ reed house (nsugo sonombu)
- ◯ timber and termite mud
- ◯ corrugated iron
- ◯ stone
- ◯ bricks

Agriculture

IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WERE GOOD HARVEST YIELDS? *

- ☐ 2007
- ☐ 2008
- ☐ 2009
- ☐ 2010
- ☐ 2011
- ☐ 2012
- ☐ 2013
- ☐ 2014
- ☐ 2015
- ☐ 2016
- ☐ 2017
- ☐ none
- ☐ don't know

6/13



Post-experimental individual survey after PG game

DO YOU HAVE ANY PLANS TO CHANGE LANDS OR EXPAND YOUR CULTIVATION AREA IN THE NEXT FIVE YEARS? *
- ◯ Yes
- ◯ No

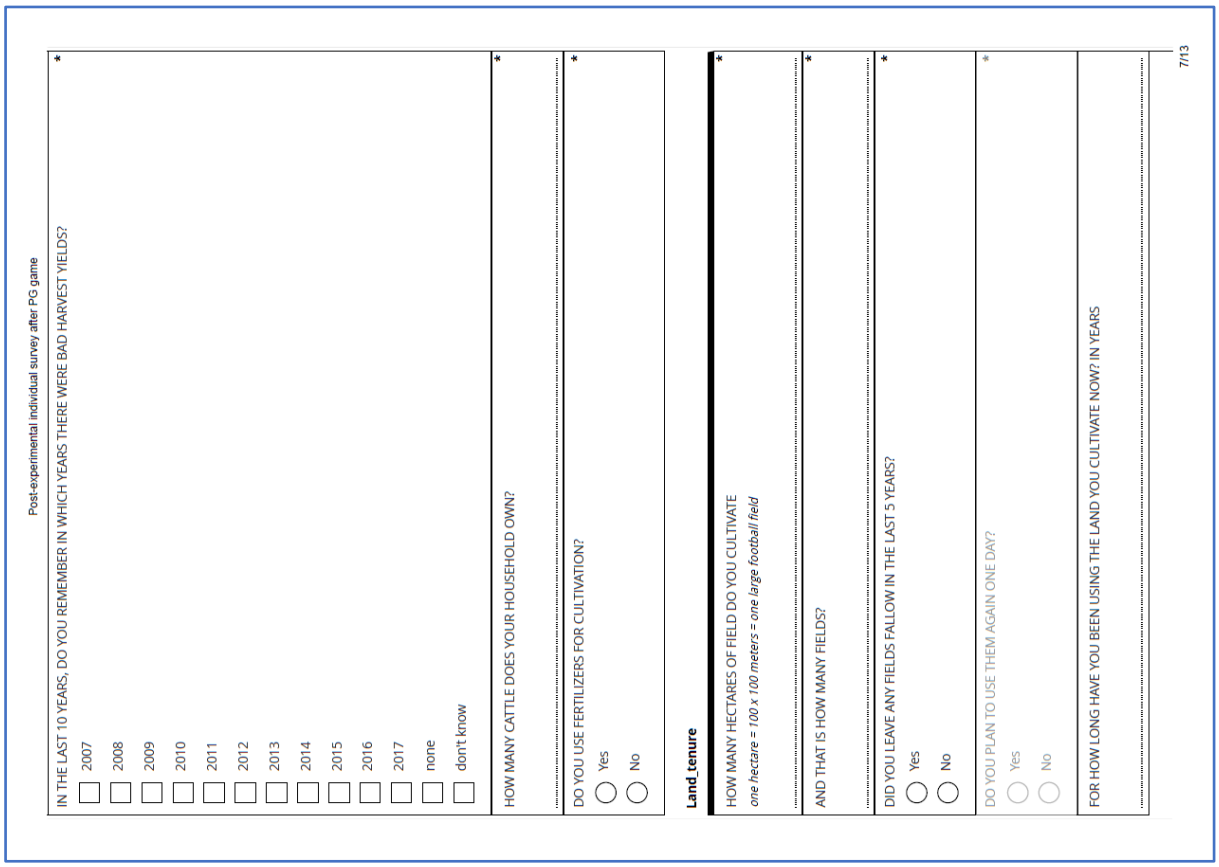
WOULD YOU SAY THERE IS SUFFICIENT LAND FOR EVERYONE? *
- ◯ Yes
- ◯ No

DO YOU REMEMBER IN WHICH YEARS YOU CLEARED FOREST FOR A NEW FIELD? *
- ☐ none
- ☐ 2000
- ☐ 2001
- ☐ 2002
- ☐ 2003
- ☐ 2004
- ☐ 2005
- ☐ 2006
- ☐ 2007
- ☐ 2008
- ☐ 2009
- ☐ 2010
- ☐ 2011
- ☐ 2012
- ☐ 2013
- ☐ 2014
- ☐ 2015
- ☐ 2016
- ☐ 2017
- ☐ never
- ☐ don't know

HOW MUCH ON AVERAGE WHEN YOU DID SO PER YEAR? *
(surface in hectare or meters, add unit!), put 0 if you they did not clear,

DO YOU NEED PERMISSION FOR CLEARING? *
- ◯ no
- ◯ yes, from headman
- ◯ Other

8/13



Post-experimental individual survey after PG game

IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WERE BAD HARVEST YIELDS? *
- ☐ 2007
- ☐ 2008
- ☐ 2009
- ☐ 2010
- ☐ 2011
- ☐ 2012
- ☐ 2013
- ☐ 2014
- ☐ 2015
- ☐ 2016
- ☐ 2017
- ☐ none
- ☐ don't know

HOW MANY CATTLE DOES YOUR HOUSEHOLD OWN? *

DO YOU USE FERTILIZERS FOR CULTIVATION? *
- ◯ Yes
- ◯ No

Land_tenure

HOW MANY HECTARES OF FIELD DO YOU CULTIVATE *
one hectare = 100 x 100 meters = one large football field

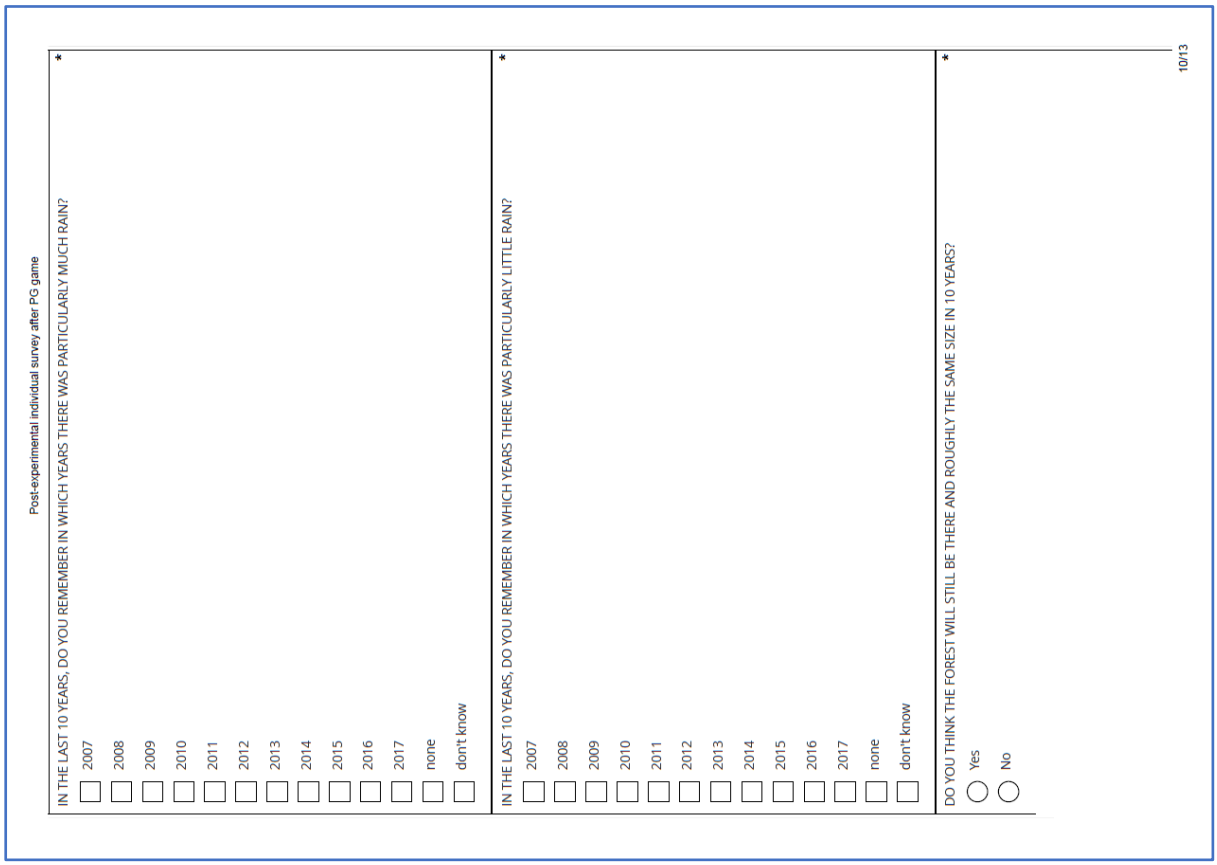
AND THAT IS HOW MANY FIELDS? *

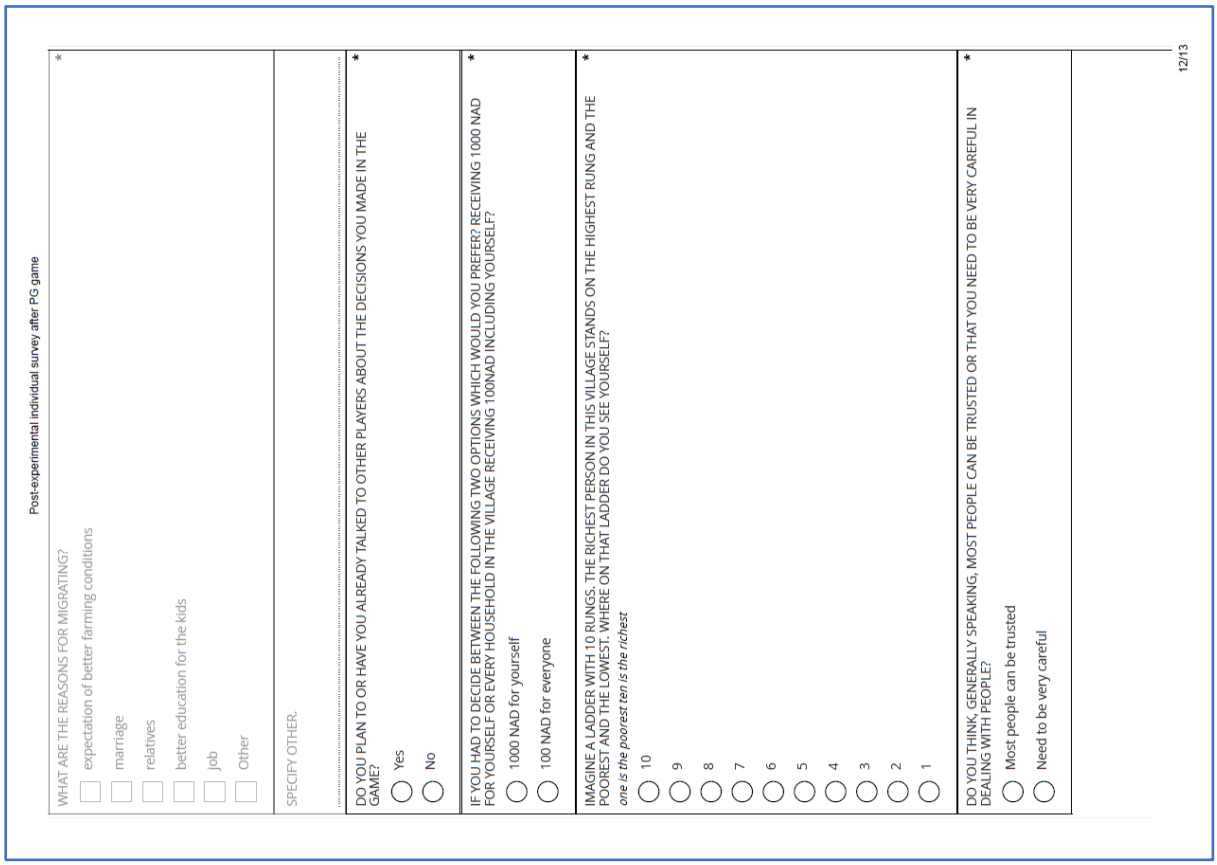
DID YOU LEAVE ANY FIELDS FALLOW IN THE LAST 5 YEARS? *
- ◯ Yes
- ◯ No

DO YOU PLAN TO USE THEM AGAIN ONE DAY? *
- ◯ Yes
- ◯ No

FOR HOW LONG HAVE YOU BEEN USING THE LAND YOU CULTIVATE NOW? IN YEARS

7/13


Post-experimental individual survey after PG game
SPECIFY OTHER.
HAVE YOU BEEN DENIED PERMISSION TO CLEAR IN THE LAST YEARS? *
Yes
No
DO YOU FEEL THERE IS SOME TYPE OF RIVALRY OR CONFLICTS IN ACQUISITION OF NEW LAND? *
Yes
No
DO YOU FEEL SAFE AND SECURE ABOUT YOUR OWN LAND (TENURE)? *
very safe
pretty safe
somewhat safe
worried
unsafe
DO YOU THINK YOU WILL STILL USE THE LAND YOU USE NOW IN 10 YEARS? *
Yes
No
Environment
HAS THERE BEEN ANY CHANGE IN THE WEATHER OVER THE LAST YEARS? *
more rain
less rain
hotter
colder
no change
don't know
9/13
Post-experimental individual survey after PG game
IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY MUCH RAIN? *
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
none
don't know
IN THE LAST 10 YEARS, DO YOU REMEMBER IN WHICH YEARS THERE WAS PARTICULARLY LITTLE RAIN? *
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
none
don't know
DO YOU THINK THE FOREST WILL STILL BE THERE AND ROUGHLY THE SAME SIZE IN 10 YEARS? *
Yes
No
10/13


Post-experimental individual survey after PG game
Extra
ARE YOU BORN IN THIS VILLAGE? *
Yes
No
FOR HOW LONG HAVE YOU LIVED IN THIS VILLAGE? *
more than 20
more than 10
more than five
less than five
WHERE DID YOU LIVE BEFORE? (OPTIONAL) *
neighbour village
far away village in Kavango
Rundu
other part in Namibia
Angola
other country
WHY DID YOU COME HERE? (OPTIONAL) *
expectation of better farming conditions
marriage
relatives
better education for the kids
job
Other
SPECIFY OTHER.
DO YOU SOMETIMES THINK ABOUT MIGRATING TO ANOTHER PLACE? *
Yes
No
WHERE WOULD YOU CONSIDER MOVING TO? *
another village
Rundu
another part of Namibia
11/13
Post-experimental individual survey after PG game
WHAT ARE THE REASONS FOR MIGRATING? *
expectation of better farming conditions
marriage
relatives
better education for the kids
job
Other
SPECIFY OTHER.
DO YOU PLAN TO OR HAVE YOU ALREADY TALKED TO OTHER PLAYERS ABOUT THE DECISIONS YOU MADE IN THE GAME? *
Yes
No
IF YOU HAD TO DECIDE BETWEEN THE FOLLOWING TWO OPTIONS WHICH WOULD YOU PREFER? RECEIVING 1000 NAD FOR YOURSELF OR EVERY HOUSEHOLD IN THE VILLAGE RECEIVING 100NAD INCLUDING YOURSELF? *
1000 NAD for yourself
100 NAD for everyone
IMAGINE A LADDER WITH 10 RUNGS. THE RICHEST PERSON IN THIS VILLAGE STANDS ON THE HIGHEST RUNG AND THE POOREST AND THE LOWEST. WHERE ON THAT LADDER DO YOU SEE YOURSELF? *
one is the poorest ten is the richest
10
9
8
7
6
5
4
3
2
1
DO YOU THINK, GENERALLY SPEAKING, MOST PEOPLE CAN BE TRUSTED OR THAT YOU NEED TO BE VERY CAREFUL IN DEALING WITH PEOPLE? *
Most people can be trusted
Need to be very careful
12/13

Supplement D: Information for data and analysis script request

Dataset and script for the data preparation and analysis (“do-file”) can be made available upon request to the corresponding author. Game protocol and instruction are also available in the local languages spoken in Kavango, Namibia.

Epilogue and Way Forward

As indicated in the introduction of this thesis, the data collected in Namibia allow producing way more than the previously presented papers. In particular, the extensive survey including a total of about 1,000 observations from rural villagers plus interviews with all villages' headpersons offers analyzing a plethora of additional scientific research topics. These do especially focus on issues of agricultural economics, economic development, climate change and land use. There are also additional data that allow looking at methodical questions, behavioral patterns and, of course, more on individual decision making in social dilemma situations.

One envisaged paper, which bases purely on real-world survey data, will be disentangling in detail factors that determine land use intensity, demand for land and crop yields. While intensification of agriculture, for example with the help of industrial fertilizers, could, in theory, work as a countermeasure to excessive extensions of used land (and therefore clearing of forests), increases in crop yields could possibly also lead to higher commercialization, mechanization and consequently, as a net effect, increases in demand for land.

Further, I plan to look at the effects of scarcity and availability of unused land, i.e., forests, using the real-world survey data as a complementary approach to my experimental study on scarcity. As mentioned in the first paper, the Kavango region offers a highly suitable background through providing both scarce environments in villages at the densely populated Kavango river, as well as abundant environments in villages of the hinterland. Migrating farmers seeking available land for agriculture could be an additional aspect to take into account with this study.

In this context, the data also allow contributing to answering the age-old question on the relationship between farm-size and productivity. It is also possible to quantitatively analyze determinants of smallholder market participation which poses an important component of agricultural and economic development. Data on climate change, variations in temperature and rainfall have also already been collected and its effects on agriculture may be considered as additional aspects.

On the methodical side, data at hand have shown a positive correlation between experimental and real-world clearing behavior, which is highly relevant insight for the use of field experiments in general. I have further collected additional measures of experimental cooperativeness with the same participants and asked several survey questions about

hypothetical cooperative and social behavior, all of which can be linked and analyzed for correlations.

The fourth paper of this thesis actually centers on only one half of the conducted public good experiment: Next to the analysis of communication effects, we also tested, with an additional treatment condition, the role of anonymity and identification in cooperation. Due to the field experimental setting, we do specifically expect stronger effects of identifying one's group members in comparison to lab experiments conducted with student samples. Further, long-term pre-existent social relationships between participants allow measuring effects of group composition and heterogeneity.

To sum up, the following list gives an overview over my current "work in progress":

- Hoenow C.; Pourviseh A.; Vollan B. *A Meta Study on Cooperation in Field Experiments*
- Hoenow C.; Kirk M. *Land Availability, Demand and Land Use Intensification. Insights from a Large-scale Survey with Local Farming Households*
- Hoenow C.; Kirk M. *Smallholder Farming, Market Participation and Climate Change in Kavango, Namibia. Survey-based Evidence*
- Hoenow C. *Field-Experimental Evidence on the Effects of Group Heterogeneity on Cooperation in Small Communities*
- Hoenow C.; Pourviseh A. *The Effect of Anonymity and Identification in a Public Good Field Experiment*
- Hoenow C. *Validity across Methods: Comparing Field-experimental Results with Real-world Behavior and Hypothetically Stated Preferences*

I hope I will be able to finish these in the future, either if my next position allows me working on them or, if that is not the case, as side projects.

Eidesstattliche Erklärung

Hiermit versichere ich an Eides statt, dass ich die vorliegende schriftliche Arbeit selbstständig und ohne fremde Hilfe verfasst, eventuelle Beitrage von Koautoren dokumentiert, keine anderen als die von mir angegebenen Hilfsmittel benutzt und alle vollständig oder sinngemäß übernommenen Zitate als solche gekennzeichnet, sowie die Dissertation in der vorliegenden oder einer ähnlichen Form noch bei keiner anderen in- oder ausländischen Hochschule anlässlich eines Promotionsgesuchs oder zu anderen Prüfungszwecken eingereicht habe.

Nils Christian Hönow, Marburg, 12.12.2020